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MASTERWORKS OF Science

DIGESTS OF 13 GREAT CLASSICS

Edited by

John Warren Knedler, Jr.

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THE EDITOR wishes to thank Peter Smith, Publisher, for permission to include our condensation of *Relativity: The Special and General Theory,* by Albert Einstein.

J. W. K., JR.

PREFACE BY THE EDITORS

THIS VOLUME is one of a series of books which will make available to the modern reader the key classics in each of the principal fields of knowledge.

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All these books have had a profound effect upon the thinking and activities of mankind. To know them is to partake of the world's great heritage of wisdom and achievement. Here, in the Masterworks Series, epoch-making ideas of past and present stand forth freshly and vividly—a modern presentation of the classics to the modern reader.

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MASTERWORKS OF SCIENCE

INTRODUCTION

MAN lives in a puzzling physical environment. Since long before the time of recorded history, he has busied himself to explain the phenomena of his world. All his theories and guesses properly belong to the history of science, even such as primitively construed the thunderbolt as a weapon in the hands of an angry, anthropomorphic god. Generally, however, only those portions of his explanations which can be organized into a coherent, self-consistent picture of his universe, and which endure the tests of observational and experimental trial, are admitted as elements in the history of science.

In this narrowed sense, science begins with the ancient Chaldeans and Egyptians. They patiently observed the changing appearance of the heavens and developed theories to account for the changes. Despite the crudity of their instruments, they made observations of astonishing acuteness. But their conclusions have not uniformly withstood the questioning of later generations. The ancient Greeks also theorized and observed. Their method—in the most general terms—was to start from certain assumptions concerning origins, and to deduce therefrom, in the most rigorously logical way, those ideas which combined to form their science. If the assumptions were successfully challenged, the whole system collapsed. Much of Greek science has from this cause been a casualty of the ages. But in geometry, an area in which deductive reasoning requires no aid, the Greek discoveries and methods have lasted. The geometers started with a few simple postulates, such as the axioms of Euclid, and deduced from them the properties of lines and points, of plane and of solid figures. Euclid (fl. 300 B.C.) combined his own geometrical work with that of his predecessors into one great edition, the *Elements*. He gave to investigators of the next two thousand years a model in the use of deductive reasoning and a form in which to present their conclusions. His is the first great name in the history of science.

About one hundred years after Euclid's death, Archimedes of Syracuse (287–212 B.C.) applied the Euclidean method to the study of levers and of hydrostatics. From a few simple axioms, always using a geometer's method, he deduced his laws. Actually it was the mathematical beauty of his problems and solutions which delighted him. The correspondence

between his conclusions and observed physical fact he considered almost incidental and immaterial. Yet his familiar laws of the lever and those of the floating body still express the world as it appears to our senses. In formulating these laws, Archimedes founded the exact science of mechanics.

Other Greek philosophers advanced larger theories to explain the universe. One of them—without convincing many of his contemporaries—arrived at what we know as the heliocentric theory; another deduced something very like the modern nebular hypothesis; another developed an idea of matter which (incidentally, Aristotle rejected it) strikingly anticipates the modern atomic theory. None of these theories won general acceptance, partly, at least, because the theorists had no means to demonstrate the validity of their ideas. The ideas themselves, even those which later investigators have revived, therefore were lost in the mass of similar notions which the Greek thinkers produced. Indeed, outside mechanics and pure mathematics, the direct debt of modern science to the Greek world is small. But modern science owes everything to the Greek idea that man can attain a generalized, rational, comprehensible explanation of the physical world. During the Middle Ages this idea lay dormant; with the Renaissance it revived. Leonardo da Vinci began freshly to observe the physical world, to question the phenomena of the world, and to experiment. So did other Italians, his contemporaries. From them Copernicus (1473–1543) caught method and enthusiasm. When he returned from Italy to Poland, principally interested in astronomy, he used the new methods of the Italian investigators. He observed, measured, theorized; then he observed and measured again to test his theories. Unable to accept the current geocentric theory, and aided by the merest hint of such a thing which had survived from the Greeks, he framed a heliocentric theory. Then, almost singlehanded, he wrought for this theory the stamp of truth.

The invention of the telescope gave Galileo (1564–1642) the opportunity to supplement the observations and measurements of Copernicus and to add to the Copernican theory the weight of evidence. He grasped even more surely than Copernicus the true method of science. Whereas his predecessors from Aristotle on had been busy, for example, with the problem of why bodies fall, Galileo set himself the more compact problem: How do bodies fall? Facing this problem, he first framed a hypothetical answer. When he found it not self-consistent, he rejected it and formed another. This process he repeated until he had a theory satisfactory to himself: that the space traversed by a falling body is proportional to the square of the time of fall. Next he devised experiments to test his theory. They confirmed it. Galileo had, partly by his discoveries, more by his methodology—theory confirmed by experiment—founded the science of dynamics.

Galileo understood and used what are now known as the first two laws of motion. The first of these is the law of inertia: a body remains in its original state of rest or of motion along a straight line unless it is acted

upon by an outside force. This law is the very foundation of dynamics. From it grows the second: the change in the velocity of a motion is proportional to the force which causes the change. Upon these two laws, the laws of inertia and of acceleration, much of later physical investigation has been based. They remain the foundation of dynamics in the gross world. Fame acknowledges Galileo for his early recognition of these laws. He deserves his niche in the history of science even more because he first saw the possibility of verifying hypotheses by experiments expressly designed for that purpose.

Born in the year of Galileo's death, Newton (1642–1727) early devoted himself to inquiries similar to those which had attracted Galileo. As a young man, he invented a method of mathematical investigation which he called "fluxions," and which modern students know as the calculus. Using it, and applying the principle of inverse squares, he extended the work of his predecessor. First he gave definitive statement to the two laws of motion already recognized, and then he added a third: to every action there is an equal and opposite reaction. He considered the problem of the universe to be a problem of matter and force—in this following Galileo—and he chose, in the manner of his predecessors, to express his findings in the form of Euclidean propositions and demonstrations. That force which causes a body to fall in the neighborhood of the earth Newton thought might operate throughout the universe. By means of the calculus —but always giving his results in the geometer's form—he satisfied himself that his idea was valid. To this force he gave the name gravitation; then he wrote mathematical equations to express gravitation and its effects. He succeeded in showing that all the motions of the heavenly bodies can be described by one simple physical law. He welded together the data of astronomy, physics, and mathematics into one great physical synthesis, one coherent system. He had done more even than Copernicus, and historians call his the greatest single achievement in the history of science.

Though physics is the aspect of science most enhanced by the labors of Euclid, Archimedes, Galileo, Copernicus, and Newton, other branches of science were not neglected during the centuries covered by the careers of these men. During the Middle Ages the alchemists strove gallantly to discover the elixir, stone, or process which would transmute one metal into another. For they were the heirs of a Greek theory that all matter is ultimately composed of one common element, and that differing substances owe their peculiar qualities to differences in the shape, size, or state of motion of particles in themselves indistinguishable from one another, of which these substances are constructed. Really the alchemists were trying to solve the problems of chemistry. Chemistry became a real science, however, only after Lavoisier, in the eighteenth century, rediscovered oxygen, produced a reasonable explanation of combustion, and by the use of the balance showed that in the course of a chemical reaction the total mass remains the same.

In the eighteenth century, geology also finally emerged from the theo-

logical bogs of the Middle Ages. When Hutton announced his uniformitarian theories, he weakened the old catastrophic theories which had been depended upon to explain the changes observable in the history of the organic and inorganic worlds. Much earlier, medicine had moved beyond the bounds of ancient knowledge. In sixteenth-century Italy, Vesalius had shown how anatomy should be studied; in seventeenth-century England, Harvey had discovered the circulation of the blood. Other physicians and surgeons in various countries of Europe had learned about the mechanisms of respiration and digestion. They had concluded that physical and chemical principles could be applied in physiology.

Considering the enormous preparatory accomplishments of these scientists—from Copernicus and Galileo to Harvey and Hutton—it is possibly not surprising that the nineteenth century surpassed all preceding centuries in the variety and magnitude of its scientific investigations and discoveries. Sir Charles Lyell (1797–1875) revolutionized geology, partly, at least, because he could take advantage of the labors of his predecessors. He exhaustively reviewed their theories, co-ordinated their dozens of studies, familiarized himself with the enormous mass of data they had accumulated on geological change. Then he threw the weight of his great learning into support of a theory largely Hutton's—that past geological changes have been brought about by natural forces still operative. Industriously, patiently, wisely, he studied subjects allied to geology, such as archeology and conchology, in order to adjust to the recognized data of observation and experiment the facets of his theory. The results convinced all geologists. By his labors Lyell immensely increased the concept of geological time. Whereas earlier commentators on the past changes in the earth's crust had imagined forces and cataclysms which might have made these changes in the course of some hundreds of years, Lyell read the geological record as the tale of millions of years. The earth, geologists and then laymen began to believe, has existed for eons. The prodigious modifications which have taken place in its crust have occurred in slow sequence.

Charles Darwin (1809–1882) could scarcely have built his theories had Lyell's concept of the extent of geological time not been previously developed. Puzzled by the variety of species, concerned over the problem of the origin of species, he saw that his problem, like Lyell's, could be solved only by one who had assembled and mastered vast bodies of information. Unlike the theorists of the earlier centuries, he found this information available. Generations of travelers and students and trained observers had bequeathed to him a volume of data which it was a life's work to assimilate. Tirelessly he labored at the task. In the quiet of a retired country life he not only experimented constantly but also read endlessly. With Lamarck's ideas and Malthus's theories as a point of departure, he had hit upon the notion that the problem of adaptation was central in the great biological puzzle of origins. So he indefatigably collected and classified data until in 1859 he was able to announce to the world, persuasively, that sexual selection, acting under the pressure of the struggle for life, ex-

plained the survival of those species favored by adaptation to their environments and the disappearance of those less fortunate. The terms of this theory, if debatable, were comprehensible; and the immense length of geological time envisaged by Lyell stretched far enough to accommodate the slow changes Darwin predicated. Within a generation the old explanations of origins, partly theological, partly folklore, had lost their standing. Darwinism was triumphant.

Scientific inquiry of all sorts might well have thrived in the nineteenth century had Darwin's ideas never been published. No doubt can exist, however, that they turned the attention of physiologists, comparative anatomists, medical scientists into those channels where industry and scientific honesty have earned the rewards the contemporary world enjoys. In Darwin's theories themselves criticism found one great flaw: if new organs and new species evolve over long spans of time, being perfect examples of adaptation only when the evolution is complete, why did they survive the earlier stages of their development when their usefulness was slight or even nonexistent? One answer to this question Mendel (1822–1884) found in his experimental work on the hybridizing of peas. He showed that discontinuous variations may arise suddenly. His results suggest that in nature sudden jumps are the normal mechanism of evolutionary development. Subsequent investigations have thrown new light upon the theories of Darwin and those of Mendel, sometimes even modifying them. There remain yet some vexed questions about heredity, environment, evolution; and vast areas interesting to the naturalist and the biologist are still unexplored. No biological scientist in recent years has been able, however, to neglect Darwin and Mendel, and their influence seems likely to last.

In its medical aspects, biological science developed gradually during the nineteenth century and more rapidly in the twentieth. Meantime, chemistry grew from almost nothing to the giant it is in the contemporary world. Lavoisier had prepared the way by showing the need for accurate measurement in all chemical experiment. But the real foundation was laid in the first decade of the nineteenth century by John Dalton (1766–1844). Considering the combinations of reagents which chemists produce in the laboratory, he observed that the proportions in these combinations are always the same. This invariability led him to his atomic theory—the idea that an element is divisible not indefinitely, but only into particles of a given size. Out of this concept came the possibility of explaining in understandable terms the recognized facts of chemical combinations. Similarly, it provided the basis for solving the riddles of gaseous volumes. Twentieth-century physics and chemistry have far outdistanced Dalton in appreciation and understanding of the almost infinitely small particles out of which matter is built. They could never have journeyed so easily and far had not Dalton paved the first street. He identified the atom as the indivisible particle of an element which cannot be modified without changing the element of which it is the constituent part. Thus he indicated to

his successors a method of defining elements in terms of their atomic properties.

A full generation after Dalton's death, Mendeleyev (1834–1907) noted that when the elements are listed in a given order, the atomic properties of these elements recur periodically. He was carrying Dalton's work a step farther. Since the older man's time the number of recognized elements had greatly increased, and as each new one was discovered, chemists had hastened to measure and record its atomic properties. Now Mendeleyev, a synthesizer, devised his great Periodic Tables. In these he showed that the known elements demand grouping in accord with their common atomic properties. His work led chemists to see greater necessity than before for the accurate determination of such properties as atomic weight. More, these Tables made possible a prophecy of then undiscovered elements, for there were blanks in the Tables. In the years since Mendeleyev first published his Tables, the blanks have gradually been filled by the discovery of elements not before known. Our own day bears witness to the excitement attending such discoveries; for plutonium and neptunium, terms easy to our lips, were strangers to yesterday's chemical vocabulary.

Concerning the phenomena of electricity, a little, but only a little, information had been collected by the end of the eighteenth century. A little —but only a little—experimental work had been done with this mysterious form of energy. Franklin's experiments with a kite, a silk string, and a door key typify at once the curiosity of his generation, the crudity of their instruments, and the state of their knowledge. Scarcely had the new century opened when imaginative experiments and startling discoveries widened the possible areas of inquiry. Volta, Ampere, Ohm are merely the better known among the many men whose labors have influenced every dweller in the twentieth century. Very great among these scientists of electricity, half chemists and half physicists, was Faraday. He is often called the "prince of experimenters," and for ingenuity in devising experiments, patience in repeating them, industry in recording them, he well deserves the title. Like Lyell and Darwin, he considered no exertion too great in the collecting of data, and like the great scientists named earlier in this essay, he recognized the value of accurate measurements. When he began his experimental labors, various electrical phenomena were already well known. He first exhaustively studied these to show that all of them witness the presence of the same energy, electricity. Then he investigated and satisfactorily explained the passage of electricity through liquids. He studied the battery and worked out a reasonable explanation of its behavior; devised means to measure electrical quantities; showed the equality between the quantity of electricity employed and the chemical action provoked in any electro-chemical process. He discovered the magnetization of light and diamagnetism; investigated magneto-crystallic action and electro-magnetic rotation; he studied the lines of magnetic force and the annual and daily variations of the magnetic needle. From among such monumental works, it is hard to choose that one which specifically marks Faraday as one of the greatest of scientists. Yet perhaps

his most important discovery was magneto-electric induction. For upon this discovery depends the operation of the dynamo, and upon the dynamo to no mean degree depends contemporary civilization.

Though his experimental work belongs almost equally to chemistry and to physics, Faraday called himself a chemist. To him physics was still the science of statics and of dynamics, founded by Galileo, brought to its peak by Newton. In the closing hours of the nineteenth century, physics and chemistry moved so close to one another that a student of one had perforce to be a student of the other. Marie Curie (1867–1934) once won the Nobel Prize as a chemist and once as a physicist. All her life long her scientific interest centered in radioactivity. In her student days the discoveries of Roentgen and Becquerel were new and exciting. She undertook to learn more about the rays they had first observed. Before her death she had almost singlehanded founded the branch of science called radioactivity, and she had herself carried far the investigations she had initiated. The discoveries she and her husband made of polonium and radium filled two of the blank spaces in Mendeleyev's tables. Her students and followers have filled many more. But it is not merely the knowledge of new elements which the scientific world owes to Marie Curie. The fruits of her studies we are only beginning to be aware of in these days of cyclotrons, atomic fission, and nuclear power.

No one needs to be reminded that the new science of radioactivity has not only wedded chemistry to physics and mathematics, but has aided medicine and biology powerfully. The twentieth century has accomplished an even more remarkable synthesis. Albert Einstein (1879–) has used the data of physics, chemistry, astronomy, and mathematics to found a new cosmogony. Trained as a mathematician and physicist, he has been able to divorce his mind from the rigid concepts of the Newtonian world and to apply freshly to the riddle of time and space the ideas available in non-Euclidean geometry, the mathematics of Minkowski, and the quantum theory of Planck. He takes neither time nor space nor motion as an absolute quantity, but maintains that all time and space and motion is relative. There is, nevertheless, in his concept a time-space continuum which has an absolute value governed by the velocity of light. In this time-space continuum bodies move along straight lines in empty space, along curved lines as they approach matter. Space-time itself curves. The General Theory of Relativity supersedes the Newtonian theory of gravitation and relegates Galileo's laws of motion to a definitive position only in special cases. When Einstein propounded his theory, many scientists hesitated long in accepting it. It was, after all, a mathematical theory, seemingly not subject to confirmation in the gross physical world. But within a few years the observations of astronomers had confirmed the new theory, and the labors of nuclear physicists had been so aided by it that it began to enjoy universal esteem. Einstein, like Copernicus and Newton, stands as the founder of a new method of approach to the problems of the physical universe. His ideas have already largely influenced the physical ideas of the twentieth century, and they will doubtless color

the philosophy the century makes its own. For like the great theories of the past, this one too is comprehensible, and it affirms once more man's inexhaustible ability to frame for himself explanations of the physical world.

When Darwin's theories had been debated and finally accepted, for a full generation biologists neglected the problems of genetics. Then the rediscovery of Mendel's work revealed to them that there were vast areas unexplored, vast empires of knowledge to be gained. After Newton's theories had won the agreement of his fellow physicists and astronomers, they so dominated physical research for two centuries that physics became practically the property of the engineer, busy solving practical problems in terms of force and matter. The general acceptance of Einstein's theory has produced no such doldrums. Rather, Einstein has reconvinced the scientific world that science is not a mere collection of laws, a series of facts, but a creation of the human mind eager to present to itself a comprehensible picture of the puzzling physical world. So long as the human mind endures, therefore, so long the scientific world will expand its boundaries. As the nineteenth century used the richly accumulated resources of preceding centuries to build a scientific structure more imposing than all preceding centuries had erected, the twentieth century bids fair to use the accomplishments of the nineteenth as the firm foundation upon which to construct a grander edifice. However daring and adventurous and successful this our century may be, it will in turn be outdone by the next.

THE ELEMENTS

by

EUCLID

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EUCLID

fl. 300 B.C.

ONE FACT is known with certainty about the Greek mathematician Euclid: he taught in Alexandria in the time of Ptolemy I and founded a school there. All other biographical details must be prefixed with a "probably." Probably he learned mathematics in Athens, probably from pupils of Plato. Several anecdotes told concerning him come from very early commentators and probably contain reflections of truth. When King Ptolemy asked him if there were no shorter way in geometry than that of the *Elements,* he replied: "There is no royal road to geometry." And when a pupil who had mastered the first proposition in the *Elements* inquired what he would get by learning such things, Euclid called a slave and instructed him to give the pupil threepence, "since he must needs make gain by what he learns."

From such biographical bits a reader may piece together a notion of Euclid as a severe but not humorless teacher, a stern, bold seeker after mathematical truth. But details about his personal and family life, about his appearance and habits, about his non-mathematical occupations and ideas, did not interest his early biographers. To learn something about Euclid, modern students must go straight to the task of studying his writings.

These writings, edited in a definitive edition in eight volumes (Heiberg and Menge, *Euclidis opera omnia,* Leipzig, 1883–1916), include the *Data, On Divisions* (of figures), *Optics, Phaenomena,* and the *Elements.* (At least four other treatises, three of them on higher geometry, have been lost.) All of these discuss problems in geometry. The *Elements* has been the standard textbook in geometry for more than two thousand years—a record unequaled by any other treatise on any subject whatsoever—and surely qualifies therefore as a Masterwork.

The *Elements* is divided into thirteen books. The first six and the last three are devoted to geometry, plane and solid; three others are devoted to arithmetic, and one is devoted to irrationals. It is the first six books which have been the study of generation after generation of schoolboys, with whom Euclid and geometry have become synonymous. But geometry is really much older than Euclid.

Geometry means "earth measurement." In the ancient world the need for earth measurements appeared acutely in Egypt because the annual floods of the Nile made surveying constantly necessary for the re-establishment of boundaries. In Egypt, therefore, a practical, applied geometry developed. It consisted of a number of crude rules for the measurement of various simple geometric figures, for laying out angles, particularly right angles, and so on. The Greeks developed this crude beginning into demonstrative geometry. That is, various mathematicians among the Greeks worked out a series of propositions so logically interrelated that if the proof of one is granted or assumed, later ones, based on it, can be proved logically from the assumptions therein demonstrated.

As early as 500 B.C., Hippocrates of Chios compiled a series of such propositions. Succeeding geometers did the same thing. Euclid analyzed the work of his predecessors, arranged the various propositions in an order of his own, introduced new proofs of some propositions, and thus composed his masterwork, the *Elements*. In the first six books about 170 geometrical propositions are presented and proved. Of these only one is certainly original with Euclid—the proof of the Pythagorean theorem, that in any right-angled triangle the square on the hypotenuse is equal to the sum of the squares on the other two sides. Yet so much needed was Euclid's editorial work that from the time of the first appearance of the *Elements,* all earlier compilations were neglected.

If geometrical propositions be arranged—as Euclid's are—in such an order that each one depends for its proof upon the acceptance of propositions earlier proved, it is evident that, proceeding backwards, one comes to an early proposition, perhaps several of them, which cannot be logical consequences of preceding ones. The logical status of these early propositions rests upon various definitions which must be precedent, and upon various assumptions or postulates or axioms the truth of which must be granted before any logical structure can be erected upon them. The first book of the *Elements* is therefore preceded by a set of definitions and a set of assumptions; and later books have, when it is necessary, similar prefaces.

The definitions seem to modern readers elementary. The axioms seem self-evident to the point that statements of them

are needless. That they are thus acceptable to us merely shows how completely our common geometric ideas stem from Euclid. For the postulates in particular, being undemonstrable, can be abandoned, and alternate or contrary postulates set up. Upon these a new geometry can be based. Several modern mathematicians have done exactly this, and from their work—notably Riemann's—comes what is known as non-Euclidean geometry.

The design of any systematic geometer must be to reduce the number of definitions and postulates to a minimum. That is, he will wish to assume as little as possible, and to force the truth of his propositions upon the reader by the might of his logic. Euclid may have originated the definitions and axioms with which his treatise begins. Possibly he rather selected from similar lists prepared by earlier geometers. Of the origin of the definitions with which the following selection begins, nothing is certainly known. Of the axioms, number 12 is acknowledged to be Euclid's.

A proposition consists of various parts. There is first the general statement of the problem or theorem, then the construction—which states the necessary straight lines and circles which must be drawn to assist in the demonstration of the theorem—and last the demonstration itself, closing Q.E.F.—*quod erat faciendum*—"which was to be constructed"—or Q.E.D.—*quod erat demonstrandum*—"which was to be proved."

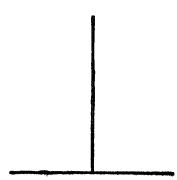
The portion of the *Elements* which follows is verbatim from the edition of Euclid prepared by Isaac Todhunter in 1862. It includes a number of the definitions and all the postulates and axioms which precede Book I; the first five propositions with their full Euclidean construction and demonstration, of which number 5 is the notorious *pons asinorum,* or bridge of asses, so called because it has ever been an obstacle to schoolboys; and number 47 from the first book, the famous Pythagorean theorem.

THE ELEMENTS

DEFINITIONS

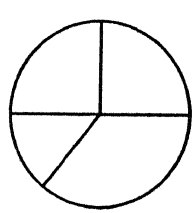
1. A point is that which has no parts, or which has no magnitude.
2. A line is length without breadth.
3. The extremities of a line are points.
4. A straight line is that which lies evenly between its extreme points.
5. A superficies is that which has only length and breadth.
6. The extremities of a superficies are lines.
7. A plane superficies is that in which any two points being taken, the straight line between them lies wholly in that superficies.
8. A plane angle is the inclination of two lines to one another in a plane, which meet together, but are not in the same direction.
9. A plane rectilineal angle is the inclination of two straight lines to one another, which meet together, but are not in the same straight line.

10. When a straight line standing on another straight line makes the adjacent angles equal to one another, each of the angles is called a right angle; and the straight line which stands on the other is called a perpendicular to it.



11. A term or boundary is the extremity of any thing.
12. A figure is that which is enclosed by one or more boundaries.

13. A circle is a plane figure contained by one line, which is called the circumference, and is such that all straight lines drawn from a certain point within the figure to the circumference are equal to one another:



14. And this point is called the centre of the circle.

15. A diameter of a circle is a straight line drawn through the centre, and terminated both ways by the circumference.

[A radius of a circle is a straight line drawn from the centre to the circumference.]

16. Rectilineal figures are those which are contained by straight lines:

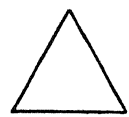
17. Trilateral figures, or triangles, by three straight lines:

18. Quadrilateral figures by four straight lines:

19. Multilateral figures, or polygons, by more than four straight lines.

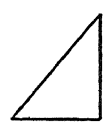
20. Of three-sided figures,

An equilateral triangle is that which has three equal sides:

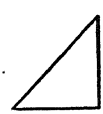


21. An isosceles triangle is that which has two sides equal:

22. A scalene triangle is that which has three unequal sides:

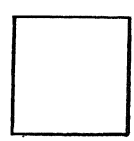


23. A right-angled triangle is that which has a right angle:

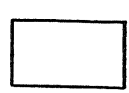


Of four-sided figures,

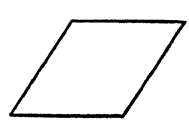
24. A square is that which has all its sides equal, and all its angles right angles:



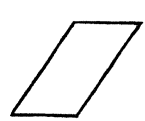
25. An oblong is that which has all its angles right angles, but not all its sides equal:



26. A rhombus is that which has all its sides equal, but its angles are not right angles:

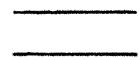


27. A rhomboid is that which has its opposite sides equal to one another, but all its sides are not equal, nor its angles right angles:



28. All other four-sided figures besides these are called trapeziums.

29. Parallel straight lines are such as are in the same plane, and which being produced ever so far both ways do not meet.



[Some writers propose to restrict the word *trapezium* to a quadrilateral which has two of its sides parallel; and it would certainly be convenient if this restriction were universally adopted.]

POSTULATES

Let it be granted,

1. That a straight line may be drawn from any one point to any other point:

2. That a terminated straight line may be produced to any length in a straight line:

3. And that a circle may be described from any centre, at any distance from that centre.

AXIOMS

1. Things which are equal to the same thing are equal to one another.

2. If equals be added to equals the wholes are equal.

3. If equals be taken from equals the remainders are equal.

4. If equals be added to unequals the wholes are unequal.

5. If equals be taken from unequals the remainders are unequal.

6. Things which are double of the same thing are equal to one another.

7. Things which are halves of the same thing are equal to one another.

8. Magnitudes which coincide with one another that is, which exactly fill the same space, are equal to one another.

9. The whole is greater than its part.

10. Two straight lines cannot enclose a space.

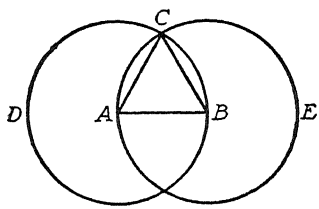
11. All right angles are equal to one another.

12. If a straight line meet two straight lines, so as to make the two interior angles on the same side of it taken together less than two right angles, these straight lines, being continually produced, shall at length meet on that side on which are the angles which are less than two right angles.

PROPOSITION 1. PROBLEM

To describe an equilateral triangle on a given finite straight line.

Let *AB* be the given straight line: it is required to describe an equilateral triangle on *AB*.



From the centre *A*, at the distance *AB*, describe the circle *BCD*. [*Postulate* 3.

From the centre *B*, at the distance *BA*, describe the circle *ACE*. [*Post.* 3.

From the point *C*, at which the circles cut one another, draw the straight lines *CA* and *CB* to the points *A* and *B*. [*Postulate* 1.

ABC shall be an equilateral triangle.

Because the point *A* is the centre of the circle *BCD*, *AC* is equal to *AB*. [*Definition* 13.

And because the point *B* is the centre of the circle *ACE*, *BC* is equal to *BA*. [*Definition* 13.

But it has been shewn that *CA* is equal to *AB*;

therefore *CA* and *CB* are each of them equal to *AB*.

But things which are equal to the same thing are equal to one another. [*Axiom* 1.

Therefore *CA* is equal to *CB*.

Therefore *CA*, *AB*, *BC* are equal to one another.

Wherefore *the triangle ABC is equilateral,* [*Definition* 20.

and it is described on the given straight line AB. Q.E.F.

PROPOSITION 2. PROBLEM

From a given point to draw a straight line equal to a given straight line.

Let *A* be the given point, and *BC* the given straight line: it is required to draw from the point *A* a straight line equal to *BC*.

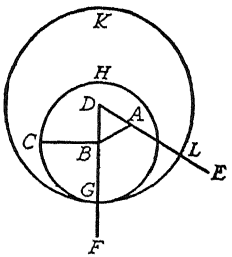
From the point *A* to *B* draw the straight line *AB*; [*Postulate* 1.

and on it describe the equilateral triangle *DAB*, [I. 1.

and produce the straight lines *DA*, *DB* to *E* and *F*. [*Postulate* 2.

From the centre *B*, at the distance *BC*, describe the circle *CGH*, meeting *DF* at *G*. [*Postulate* 3.

From the centre *D*, at the distance *DG*, describe the circle *GKL*, meeting *DE* at *L*. [*Postulate* 3.
AL shall be equal to *BC*.



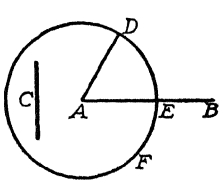
Because the point *B* is the centre of the circle *CGH*, *BC* is equal to *BG*. [*Definition* 13.
And because the point *D* is the centre of the circle *GKL*, *DL* is equal to *DG*; [*Definition* 13.
and *DA*, *DB* parts of them are equal; [*Definition* 20.
therefore the remainder *AL* is equal to the remainder *BG*. [*Axiom* 3.
But it has been shewn that *BC* is equal to *BG*;
therefore *AL* and *BC* are each of them equal to *BG*.
But things which are equal to the same thing are equal to one another. [*Axiom* 1.
Therefore *AL* is equal to *BC*.

Wherefore *from the given point A a straight line AL has been drawn equal to the given straight line BC*. Q.E.F.

PROPOSITION 3. PROBLEM

From the greater of two given straight lines to cut off a part equal to the less.

Let *AB* and *C* be the two given straight lines, of which *AB* is the greater: it is required to cut off from *AB*, the greater, a part equal to *C* the less.



From the point *A* draw the straight line *AD* equal to *C*; [I. 2.
and from the centre *A*, at the distance *AD*, describe the circle *DEF* meeting *AB* at *E*. [*Postulate* 3.
AE shall be equal to *C*.

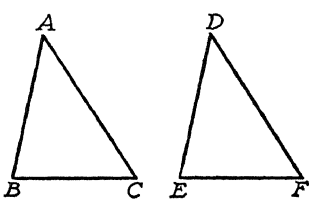
Because the point *A* is the centre of the circle *DEF*, *AE* is equal to *AD*. [*Definition* 13.
But *C* is equal to *AD*. [*Construction.*
Therefore *AE* and *C* are each of them equal to *AD*.
Therefore *AE* is equal to *C*. [*Axiom* 1.

Wherefore *from AB the greater of two given straight lines a part AE has been cut off equal to C the less.* Q.E.F.

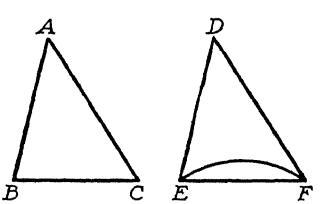
PROPOSITION 4. THEOREM

If two triangles have two sides of the one equal to two sides of the other, each to each, and have also the angles contained by those sides equal to one another, they shall also have their bases or third sides equal; and the two triangles shall be equal, and their other angles shall be equal, each to each, namely those to which the equal sides are opposite.

Let *ABC*, *DEF* be two triangles which have the two sides *AB*, *AC* equal to the two sides *DE*, *DF*, each to each, namely, *AB* to *DE*, and *AC* to *DF*, and the angle *BAC* equal to the angle *EDF*: the base *BC* shall be equal to the base *EF*, and the triangle *ABC* to the triangle *DEF*, and the other angles shall be equal, each to each, to which the equal sides are opposite, namely, the angle *ABC* to the angle *DEF*, and the angle *ACB* to the angle *DFE*.



For if the triangle *ABC* be applied to the triangle *DEF*, so that the point *A* may be on the point *D*, and the straight line *AB* on the straight line *DE*, the point *B* will coincide with the point *E*, because *AB* is equal to *DE*. [*Hypothesis.*
And, *AB* coinciding with *DE*, *AC* will fall on *DF*, because the angle *BAC* is equal to the angle *EDF*. [*Hypothesis.*
Therefore also the point *C* will coincide with the point *F*, because *AC* is equal to *DF*. [*Hypothesis.*



But the point *B* was shewn to coincide with the point *E*, therefore the base *BC* will coincide with the base *EF*;
because, *B* coinciding with *E* and *C* with *F*, if the base *BC* does not coincide with the base *EF*, two straight lines will enclose a space; which is impossible. [*Axiom* 10.
Therefore the base *BC* coincides with the base *EF*, and is equal to it. [*Axiom* 8.
Therefore the whole triangle *ABC* coincides with the whole triangle *DEF*, and is equal to it. [*Axiom* 8.
And the other angles of the one coincide with the other angles of the other, and are equal to them, namely, the angle *ABC* to the angle *DEF*, and the angle *ACB* to the angle *DFE*.

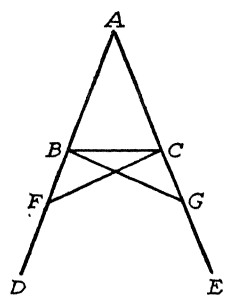
Wherefore, *if two triangles* &c. Q.E.D.

PROPOSITION 5. THEOREM

The angles at the base of an isosceles triangle are equal to one another; and if the equal sides be produced the angles on the other side of the base shall be equal to one another.

Let *ABC* be an isosceles triangle, having the side *AB* equal to the side *AC*, and let the straight lines *AB*, *AC* be produced to *D* and *E*: the angle *ABC* shall be equal to the angle *ACB*, and the angle *CBD* to the angle *BCE*.

In *BD* take any point *F*,
and from *AE* the greater cut off *AG* equal to *AF* the less, [I. 3.
and join *FC*, *GB*.



Because *AF* is equal to *AG*, [*Construction*.
and *AB* to *AC*, [*Hypothesis*.
the two sides *FA*, *AC* are equal to the two sides *GA*, *AB*, each to each; and they contain the angle *FAG* common to the two triangles *AFC*, *AGB*; therefore the base *FC* is equal to the base *GB*, and the triangle *AFC* to the triangle *AGB*, and the remaining angles of the one to the remaining angles of the other, each to each, to which the equal sides are opposite, namely the angle *ACF* to the angle *ABG*, and the angle *AFC* to the angle *AGB*. [I. 4.

And because the whole *AF* is equal to the whole *AG*, of which the parts *AB*, *AC* are equal, [*Hypothesis.*
the remainder *BF* is equal to the remainder *CG*. [*Axiom* 3.
And *FC* was shewn to be equal to *GB*;
therefore the two sides *BF*, *FC* are equal to the two sides *CG*, *GB*, each to each;
and the angle *BFC* was shewn to be equal to the angle *CGB*; therefore the triangles *BFC*, *CGB* are equal, and their other angles are equal, each to each, to which the equal sides are opposite, namely the angle *FBC* to the angle *GCB*, and the angle *BCF* to the angle *CBG*. [I. 4.

And since it has been shewn that the whole angle *ABG* is equal to the whole angle *ACF*,
and that the parts of these, the angles *CBG*, *BCF* are also equal;
therefore the remaining angle *ABC* is equal to the remaining angle *ACB*, which are the angles at the base of the triangle *ABC*. [*Axiom* 3.

And it has also been shewn that the angle *FBC* is equal to the angle *GCB*, which are the angles on the other side of the base.

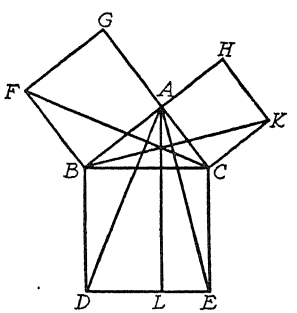
Wherefore, *the angles* &c. Q.E.D.

Corollary. Hence every equilateral triangle is also equiangular.

PROPOSITION 47. THEOREM

In any right-angled triangle, the square which is described on the side subtending the right angle is equal to the squares described on the sides which contain the right angle.

Let *ABC* be a right-angled triangle, having the right angle *BAC:* the square described on the side *BC* shall be equal to the squares described on the sides *BA*, *AC*.



On *BC* describe the square *BDEC*, and on *BA*, *AC* describe the squares *GB*, *HC*;
through *A* draw *AL* parallel to *BD* or *CE*;
and join *AD*, *FC*.

Then, because the angle *BAC* is a right angle, [*Hypothesis.*
and that the angle *BAG* is also a right angle, [*Definition* 24.

the two straight lines *AC, AG*, on the opposite sides of *AB*, make with it at the point *A* the adjacent angles equal to two right angles;
therefore *CA* is in the same straight line with *AG*.
For the same reason, *AB* and *AH* are in the same straight line.

Now the angle *DBC* is equal to the angle *FBA*, for each of them is a right angle. [*Axiom* 11.
Add to each the angle *ABC*.
Therefore the whole angle *DBA* is equal to the whole angle *FBC*. [*Axiom* 2.
And because the two sides *AB, BD* are equal to the two sides *FB, BC*, each to each; [*Definition* 24.
and the angle *DBA* is equal to the angle *FBC*;
therefore the triangle *ABD* is equal to the triangle *FBC*. [I. 4.

Now the parallelogram *BL* is double of the triangle *ABD*, because they are on the same base *BD*, and between the same parallels *BD, AL*. [I. 41.
And the square *GB* is double of the triangle *FBC*, because they are on the same base *FB*, and between the same parallels *FB, GC*. [I. 41.
But the doubles of equals are equal to one another. [*Axiom* 6.
Therefore the parallelogram *BL* is equal to the square *GB*.

In the same manner, by joining *AE, BK*, it can be shewn, that the parallelogram *CL* is equal to the square *CH*. Therefore the whole square *BDEC* is equal to the two squares *GB, HC*. [*Axiom* 2.
And the square *BDEC* is described on *BC*, and the squares *GB, HC* on *BA, AC*.
Therefore the square described on the side *BC* is equal to the squares described on the sides *BA, AC*.

Wherefore, *in any right-angled triangle* &c. Q.E.D.

ON FLOATING BODIES, AND OTHER PROPOSITIONS

by

ARCHIMEDES

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ARCHIMEDES

287–212 B.C.

ARCHIMEDES was born too late to study under Euclid. But when, as a young man, he went to Alexandria to study, his instructors in mathematics there were students and successors of Euclid. Ever afterward he considered himself a geometer. Physicists remember him for his investigations into the behavior of floating bodies and for his studies of the lever. Historians mention his invention of military engines used by his kinsman, Hieron of Syracuse, to stave off the besieging Romans. He himself regarded his practical inventions and his mechanical inquiries as the "diversions of geometry at play." Plutarch reports of him that he "possessed so lofty a spirit, so profound a soul, and such a wealth of scientific knowledge that . . . he would not consent to leave behind him any written work on such subjects, but, regarding as ignoble and sordid the business of mechanics and every sort of art which is directed to practical utility, he placed his whole ambition in those speculations in the beauty and subtlety of which there is no admixture of the common needs of life." It is recorded that he wished to have placed on his tomb a representation of a cylinder circumscribing a sphere within it, together with an inscription giving the ratio 3/2 which the cylinder's volume bears to the sphere's. Apparently he considered the discovery of this mathematical relationship to be his great claim upon posterity's regard.

The episodes of Archimedes' life cannot clearly be read in the conflicting accounts which give any information about him. After the years of study in Egypt he returned to the Greek city of Syracuse in Sicily, his birthplace, there to spend his days in studying geometry save when, at the command of the king, he did occasionally apply himself to mechanics. He was killed when the Romans finally took Syracuse and sacked it. A picturesque version of his death says that while

he was working over an intricate geometrical diagram, a Roman soldier came too close. Archimedes ordered: "Stand aside, fellow, from my diagram!" Immediately the conquering soldier, in a rage, killed him. If the story is not true, it at least underlines the notion elsewhere derived that Archimedes died, as he had lived, in the midst of mathematical speculation.

Unlike Euclid, Archimedes was not a compiler of geometrical propositions and an editor of the work of others. Rather, taking the work of others as completed, he embarked on new inquiries based on what they had accomplished. He remarks in one of his letters that, in connection with the attempts of earlier geometers to square the circle, he noticed that no one had tried to square a parabolic segment. Taking the problem for his own, he eventually solved it. In the preface to one of his works he reviews the theorems of a predecessor, Eudoxus, about the pyramid, cone, and cylinder, and approves them. Then he offers, as supplements to the work of Eudoxus, his own greater discoveries about the relative surfaces and volumes of cylinders and spheres.

The works of Archimedes—so far as they remain to us—include two books on the sphere and cylinder, two on plane equilibriums, two on floating bodies, one each on spirals, on conoids and spheroids, on the parabola, and on the measurement of the circle. There is a work called *Method* in which he tells, in the form of a letter to a friend, how he generally conceived of a theorem by means of mechanics and then proceeded to a rigorous geometrical proof of it. And another work, *The Sand Reckoner,* is a curiosity of mathematics, invaluable to our knowledge of Greek astronomy by reason of the materials it uses, and fascinating because it reveals the versatility and ingenuity of Archimedes. It begins with the observation that the sands have been called innumerable chiefly because sufficiently large numbers do not exist to record their numbers. Then, assuming that the whole universe is compact of sand, Archimedes shows that a system of numbers can readily be formed to express the total. His method amounts to our modern one of expressing large numbers as powers of ten. But the Greeks used letters and words, not numerals, to express numbers. Archimedes had, therefore, to invent a method of "orders" and "periods" so that he could write the higher powers of numbers. He thus succeeds in expressing in a few words any number up to that which in modern notation would be written as 1 followed by 80,000 billion ciphers.

Various references, many of them Arabian, indicate that Archimedes composed other works than those listed. Though

he did live a long span, it is hard to understand where, in a lifetime so productive of mathematical masterpieces, he found time and energy to perfect also the mechanical devices, methods, and principles for which the non-mathematical world reveres him. Historians of science call him the greatest mathematician of antiquity, perhaps the greatest mathematical genius of all time. They admire him for his application of the principle of exhaustion to geometrical measurement, a practice in which he anticipates the calculus of Leibnitz and Newton. Less specialized historians remember his work on levers, his invention of war machines for hurling missiles, his experiments to discover whether the king's crown were pure gold or a mixture of gold and silver—an experiment in which he evolved a method for measuring specific gravity. Every schoolboy knows the story, possibly true, of how, in his excitement over solving a problem which he had been pondering while he bathed, he ran naked through the streets shouting "Eureka"—that is, "I've got it."

Of the mechanical appliances which Archimedes invented, there is no record in his own words. Of his work on levers, floating bodies, and so on, there remains a series of theorems and demonstrations which constantly indicate that he had learned his method of rigorous mathematical proof from Euclid's *Elements*. In fact, so precisely does he apply the Euclidean method that frequently a reader does not understand as he reads an initial theorem whither it will lead. For example, the second theorem on Floating Bodies proves that the surface of any fluid at rest is the surface of a sphere the center of which is the center of the earth. Then in logical order follow four theorems devoted to the behavior of solids placed in liquids. Finally, at Proposition 7, occurs the statement now known to us as Archimedes' principle—that a solid immersed in a fluid is buoyed up by a force equal to the weight of the fluid displaced. Plutarch remarks that it is not possible "to find in geometry more difficult and troublesome questions, or more simple and more lucid explanations." The lucidity and simplicity, all editors agree, is a real miracle of workmanship.

In geometry, Archimedes built upon the work of his predecessors. In mechanics, and particularly in hydrostatics, he was a wholly original workman. He had the ability to see a problem in all its difficulties, to plan an attack upon it, and —so far as records show—always to conquer the obstacles in the way of a solution. Yet he was honest and modest enough to make a great point in one of his prefaces of confessing that certain views he had previously held were in error. He thus presents to posterity the picture of the perfect scientist—one

original, rigorous, pertinacious, and, equally important, modest and honest. It is no wonder that his name lives.

The passages from Archimedes' works which follow are from the translation of T. L. Heath.

ON FLOATING BODIES, AND OTHER PROPOSITIONS

ON THE SPHERE AND CYLINDER

"ARCHIMEDES to Dositheus greeting.

On a former occasion I sent you the investigations which I had up to that time completed, including the proofs, showing that any segment bounded by a straight line and a section of a right-angled cone [a parabola] is four-thirds of the triangle which has the same base with the segment and equal height. Since then certain theorems not hitherto demonstrated (*ἀνελέγκτων*) have occurred to me, and I have worked out the proofs of them. They are these: first, that the surface of any sphere is four times its greatest circle (*τοῦ μεγίστου κύκλου*); next, that the surface of any segment of a sphere is equal to a circle whose radius (*ἡ ἐκ τοῦ κέντρου*) is equal to the straight line drawn from the vertex (*κορυφή*) of the segment to the circumference of the circle which is the base of the segment; and, further, that any cylinder having its base equal to the greatest circle of those in the sphere, and height equal to the diameter of the sphere, is itself [*i.e.* in content] half as large again as the sphere, and its surface also [including its bases] is half as large again as the surface of the sphere. Now these properties were all along naturally inherent in the figures referred to (*αὐτῃ τῃ φύσει προυπῆρχεν περὶ τὰ εἰρημένα σχήματα*), but remained unknown to those who were before my time engaged in the study of geometry. Having, however, now discovered that the properties are true of these figures, I cannot feel any hesitation in setting them side by side both with my former investigations and with those of the theorems of Eudoxus on solids which are held to be most irrefragably established, namely, that any pyramid is one third part of the prism which has the same base with the pyramid and equal height, and that any cone is one third part of the cylinder which has the same base with the cone and equal height. For, though these properties also were naturally inherent in the figures all along, yet they were in fact unknown to all the many able geometers who lived before Eudoxus, and had not been observed by any one. Now, however, it will be open to those who possess the requisite ability to examine these discoveries of mine. They ought to have been published while Conon was still alive, for I should conceive

that he would best have been able to grasp them and to pronounce upon them the appropriate verdict; but, as I judge it well to communicate them to those who are conversant with mathematics, I send them to you with the proofs written out, which it will be open to mathematicians to examine. Farewell.

I first set out the assumptions which I have used for the proofs of my proposition.

Assumptions

1. Of all lines which have the same extremities the straight line is the least.

2. Of other lines in a plane and having the same extremities, [any two] such are unequal whenever both are concave in the same direction and one of them is either wholly included between the other and the straight line which has the same extremities with it, or is partly included by, and is partly common with, the other; and that [line] which is included is the lesser [of the two].

3. Similarly, of surfaces which have the same extremities, if those extremities are in a plane, the plane is the least [in area].

4. Of other surfaces with the same extremities, the extremities being in a plane, [any two] such are unequal whenever both are concave in the same direction and one surface is either wholly included between the other and the plane which has the same extremities with it, or is partly included by, and partly common with, the other; and that [surface] which is included is the lesser [of the two in area].

5. Further, of unequal lines, unequal surfaces, and unequal solids, the greater exceeds the less by such a magnitude as, when added to itself, can be made to exceed any assigned magnitude among those which are comparable with [it and with] one another.

These things being premised, *if a polygon be inscribed in a circle, it is plain that the perimeter of the inscribed polygon is less than the circumference of the circle;* for each of the sides of the polygon is less than that part of the circumference of the circle which is cut off by it.

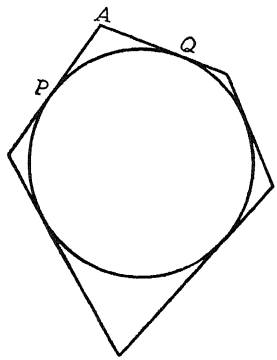
Proposition

If a polygon be circumscribed about a circle, the perimeter of the circumscribed polygon is greater than the perimeter of the circle.

Let any two adjacent sides, meeting in A, touch the circle at P, Q respectively.

Then [*Assumptions*, 2]

$$PA+AQ>(\text{arc } PQ).$$



A similar inequality holds for each angle of the polygon; and, by addition, the required result follows.

MEASUREMENT OF A CIRCLE

Proposition 1

The area of any circle is equal to a right-angled triangle in which one of the sides about the right angle is equal to the radius, and the other to the circumference, of the circle.

Proposition 2

The area of a circle is to the square on its diameter as 11 *to* 14.

Proposition 3

The ratio of the circumference of any circle to its diameter is less than $3\frac{1}{7}$ *but greater than* $3\frac{10}{71}$.

I. Let AB be the diameter of any circle, O its centre, AC the tangent at A; and let the angle AOC be one-third of a right angle.

Then $OA:AC > 265:153$ ..... (1),

and $OC:CA = 306:153$ ..... (2).

First, draw OD bisecting the angle AOC and meeting AC in D.

Now $CO:OA = CD:DA$, [Eucl. VI. 3]

so that $[CO+OA:OA = CA:DA$; or$]$

$CO+OA:CA = OA:AD$.

Therefore by (1) and (2)

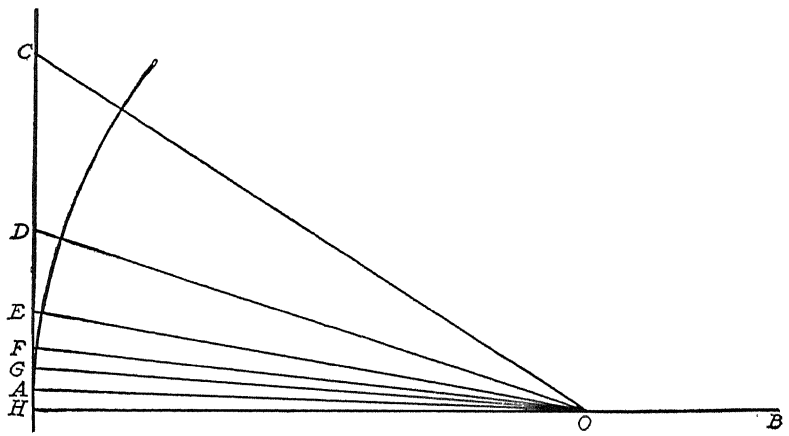
$$OA:AD > 571:153 \quad \ldots\ldots (3).$$

Hence $OD^2:AD^2 = (OA^2+AD^2):AD^2$

$$> (571^2+153^2):153^2$$

$$> 349450:23409,$$

so that $OD:DA > 591\frac{1}{8}:153 \quad \ldots\ldots (4).$



Secondly, let OE bisect the angle AOD, meeting AD in E.

Then $DO:OA = DE:EA,$

so that $DO+OA:DA = OA:AE.$

Therefore $OA:AE > 1162\frac{1}{8}:153 \quad \ldots\ldots (5).$

It follows that

$$OE^2:EA^2 > \{(1162\tfrac{1}{8})^2+153^2\}:153^2$$

$$> (1350534\tfrac{33}{64}+23409):23409$$

$$> 1373943\tfrac{33}{64}:23409.$$

Thus $OE:EA > 1172\frac{1}{8}:153 \quad \ldots\ldots (6).$

Thirdly, let OF bisect the angle AOE and meet AE in F.

We thus obtain the result corresponding to (3) and (5) above that

$$OA:AF[> (1162\tfrac{1}{8}+1172\tfrac{1}{8}):153]$$

$$> 2334\tfrac{1}{4}:153 \quad \ldots\ldots (7).$$

Therefore $OF^2:FA^2 > \{(2334\tfrac{1}{4})^2+153^2\}:153^2$

$$> 5472132\tfrac{1}{16}:23409.$$

Thus $OF:FA > 2339\frac{1}{4}:153 \quad \ldots\ldots (8).$

Fourthly, let OG bisect the angle AOF, meeting AF in G.

We have then

$OA:AG > (2334\frac{1}{4}+2339\frac{1}{4}):153$, by means of (7) and (8)
$> 4673\frac{1}{2}:153$.

Now the angle AOC, which is one-third of a right angle, has been bisected four times, and it follows that

$$\angle AOG = \tfrac{1}{48} \text{ (a right angle)}.$$

Make the angle AOH on the other side of OA equal to the angle AOG, and let GA produced meet OH in H.

Then $\angle GOH = \frac{1}{24}$ (a right angle).

Thus GH is one side of a regular polygon of 96 sides circumscribed to the given circle.

And, since $OA:AG > 4673\frac{1}{2}:153$,

while $AB = 2OA$, $GH = 2AG$,

it follows that

AB:(perimeter of polygon of 96 sides) $[> 4673\frac{1}{2}:153 \times 96]$
$> 4673\frac{1}{2}:14688$.

But
$$\frac{14688}{4673\frac{1}{2}} = 3 + \frac{667\frac{1}{2}}{4673\frac{1}{2}}$$
$$< 3 + \frac{667\frac{1}{2}}{4672\frac{1}{2}}$$
$$< 3\tfrac{1}{7}.$$

Therefore the circumference of the circle (being less than the perimeter of the polygon) is *a fortiori* less than $3\frac{1}{7}$ times the diameter AB.

II. Next let AB be the diameter of a circle, and let AC, meeting the circle in C, make the angle CAB equal to one-third of a right angle. Join BC.

Then $AC:CB < 1351:780$.

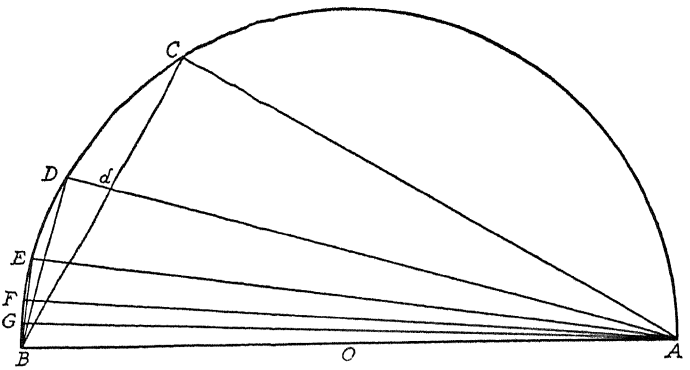
First, let AD bisect the angle BAC and meet BC in d and the circle in D. Join BD.

Then
$$\angle BAD = \angle dAC$$
$$= \angle dBD,$$

and the angles at D, C are both right angles.

It follows that the triangles ADB, $[ACd]$, BDd are similar.

Therefore
$$AD:DB = BD:Dd$$
$$= AB:Bd \qquad \text{[Eucl. VI. 3]}$$
$$= AB+AC:Bd+Cd$$
$$= AB+AC:BC$$



or $BA+AC:BC=AD:DB.$

But $AC:CB<1351:780$, from above,

while $BA:BC=2:1$

$=1560:780.$

Therefore $AD:DB<2911:780$ ..... (1).

Hence $AE^2:BD^2<(2911^2+780^2):780^2$

$<9082321:608400.$

Thus $AB:BD<3013\frac{3}{4}:780$ ..... (2).

Secondly, let AE bisect the angle BAD, meeting the circle in E; and let BE be joined.

Then we prove, in the same way as before, that

$AE:EB=BA+AD:BD$

$<(3013\frac{3}{4}+2911):780$, by (1) and (2)

$<5924\frac{3}{4}:780$

$<5924\frac{3}{4}\times\frac{4}{13}:780\times\frac{4}{13}$

$<1823:240$ ..... (3).

Hence $AB^2:BE^2<(1823^2+240^2):240^2$

$<3380929:57600.$

Therefore $AB:BE<1838\frac{9}{11}:240$ ..... (4).

Thirdly, let AF bisect the angle BAE, meeting the circle in F.

Thus $AF:FB=BA+AE:BE$

$<3661\frac{9}{11}:240$, by (3) and (4)

$<3661\frac{9}{11}\times\frac{11}{40}:240\times\frac{11}{40}$

$<1007:66$ ..... (5).

It follows that

$AB^2:BF^2<(1007^2+66^2):66^2$

$<1018405:4356.$

Therefore $AB:BF < 1009\frac{1}{6}:66$ ..... (6).

Fourthly, let the angle BAF be bisected by AG meeting the circle in G.

Then $AG:GB = BA+AF:BF$

$< 2016\frac{1}{6}:66$, by (5) and (6).

And $AB^2:BG^2 < \{(2016\frac{1}{6})^2+66^2\}:66^2$

$< 4069284\frac{1}{36}:4356$.

Therefore $AB:BG < 2017\frac{1}{4}:66$,

whence $BG:AB > 66:2017\frac{1}{4}$ ..... (7).

Now the angle BAG which is the result of the fourth bisection of the angle BAC, or of one-third of a right angle, is equal to one-forty-eighth of a right angle.

Thus the angle subtended by BG at the centre is

$\frac{1}{24}$ (a right angle).

Therefore BG is a side of a regular inscribed polygon of 96 sides.

It follows from (7) that

(perimeter of polygon):AB [$> 96 \times 66:2017\frac{1}{4}$]

$> 6336:2017\frac{1}{4}$.

And $\frac{6336}{2017\frac{1}{4}} > 3\frac{10}{71}$.

Much more then is the circumference of the circle greater than $3\frac{10}{71}$ times the diameter.

Thus the ratio of the circumference to the diameter

$< 3\frac{1}{7}$ but $> 3\frac{10}{71}$.

ON THE EQUILIBRIUM OF PLANES OR THE CENTRES OF GRAVITY OF PLANES

I POSTULATE the following:

1. Equal weights at equal distances are in equilibrium, and equal weights at unequal distances are not in equilibrium but incline towards the weight which is at the greater distance.

2. If, when weights at certain distances are in equilibrium, something be added to one of the weights, they are not in equilibrium but incline towards that weight to which the addition was made.

3. Similarly, if anything be taken away from one of the weights, they are not in equilibrium but incline towards the weight from which nothing was taken.

4. When equal and similar plane figures coincide if applied to one another, their centres of gravity similarly coincide.

5. In figures which are unequal but similar the centres of gravity will be similarly situated. By points similarly situated in relation to similar figures I mean points such that, if straight lines be drawn from them to the equal angles, they make equal angles with the corresponding sides.

6. If magnitudes at certain distances be in equilibrium, (other) magnitudes equal to them will also be in equilibrium at the same distances.

7. In any figure whose perimeter is concave in (one and) the same direction the centre of gravity must be within the figure.

Proposition 1

Weights which balance at equal distances are equal.

For, if they are unequal, take away from the greater the difference between the two. The remainders will then not balance [*Post.* 3]; which is absurd.

Therefore the weights cannot be unequal.

Proposition 2

Unequal weights at equal distances will not balance but will incline towards the greater weight.

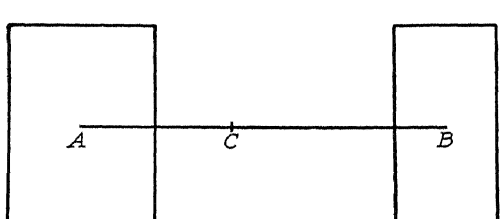
For take away from the greater the difference between the two. The equal remainders will therefore balance [*Post.* 1]. Hence, if we add the difference again, the weights will not balance but incline towards the greater [*Post.* 2].

Proposition 3

Unequal weights will balance at unequal distances, the greater weight being at the lesser distance.

Let A, B be two unequal weights (of which A is the greater) balancing about C at distances AC, BC respectively.

Then shall AC be less than BC. For, if not, take away from A the weight $(A—B)$. The remainders will then incline towards B [*Post.* 3]. But



this is impossible, for (1) if $AC=CB$, the equal remainders will balance, or (2) if $AC>CB$, they will incline towards A at the greater distance [*Post.* 1].

Hence $AC<CB$.

Conversely, if the weights balance, and $AC<CB$, then $A>B$.

Proposition 4

If two equal weights have not the same centre of gravity, the centre of gravity of both taken together is at the middle point of the line joining their centres of gravity.

Proposition 5

If three equal magnitudes have their centres of gravity on a straight line at equal distances, the centre of gravity of the system will coincide with that of the middle magnitude.

COR. 1. *The same is true of any odd number of magnitudes if those which are at equal distances from the middle one are equal, while the distances between their centres of gravity are equal.*

COR. 2. *If there be an even number of magnitudes with their centres of gravity situated at equal distances on one straight line, and if the two middle ones be equal, while those which are equidistant from them (on each side) are equal respectively, the centre of gravity of the system is the middle point of the line joining the centres of gravity of the two middle ones.*

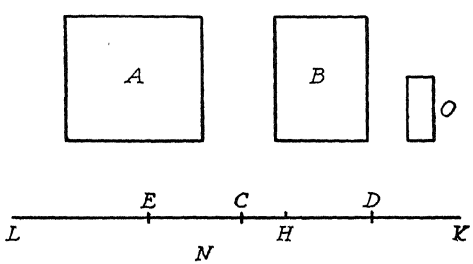
Proposition 6

Two magnitudes balance at distances reciprocally proportional to the magnitudes.

I. Suppose the magnitudes A, B to be commensurable, and the points A, B to be their centres of gravity. Let DE be a straight line so divided at C that

$$A:B=DC:CE.$$

We have then to prove that, if A be placed at E and B at D, C is the centre of gravity of the two taken together.



Since *A, B* are commensurable, so are *DC, CE*. Let *N* be a common measure of *DC, CE*. Make *DH, DK* each equal to *CE*, and *EL* (on *CE* produced) equal to *CD*. Then *EH=CD*, since *DH=CE*. Therefore *LH* is bisected at *E*, as *HK* is bisected at *D*.

Thus *LH, HK* must each contain *N* an even number of times.

Take a magnitude *O* such that *O* is contained as many times in *A* as *N* is contained in *LH*, whence

$$A:O=LH:N.$$

But

$$B:A=CE:DC$$
$$=HK:LH.$$

Hence, *ex aequali*, $B:O=HK:N$, or *O* is contained in *B* as many times as *N* is contained in *HK*.

Thus *O* is a common measure of A, B.

Divide *LH, HK* into parts each equal to *N*, and *A, B* into parts each equal to *O*. The parts of *A* will therefore be equal in number to those of *LH*, and the parts of *B* equal in number to those of *HK*. Place one of the parts of *A* at the middle point of each of the parts *N* of *LH*, and one of the parts of *B* at the middle point of each of the parts *N* of *HK*.

Then the centre of gravity of the parts of *A* placed at equal distances on *LH* will be at *E*, the middle point of *LH* [*Prop.* 5, *Cor.* 2], and the centre of gravity of the parts of *B* placed at equal distances along *HK* will be at *D*, the middle point of *HK*.

Thus we may suppose *A* itself applied at *E*, and *B* itself applied at *D*.

But the system formed by the parts *O* of *A* and *B* together is a system of equal magnitudes even in number and placed at equal distances along *LK*. And, since *LE=CD*, and *EC=DK*, *LC=CK*, so that *C* is the middle point of *LK*. Therefore *C* is the centre of gravity of the system ranged along *LK*.

Therefore *A* acting at *E* and *B* acting at *D* balance about the point *C*.

ON FLOATING BODIES

Postulate

"Let it be supposed that a fluid is of such a character that, its parts lying evenly and being continuous, that part which is thrust the less is driven along by that which is thrust the more; and that each of its parts is thrust by the fluid which is above it in a perpendicular direction if the fluid be sunk in anything and compressed by anything else."

Proposition 1

If a surface be cut by a plane always passing through a certain point, and if the section be always a circumference of a circle whose centre is the aforesaid point, the surface is that of a sphere.

For, if not, there will be some two lines drawn from the point to the surface which are not equal.

Suppose *O* to be the fixed point, and *A, B* to be two points on the surface such that *OA, OB* are unequal. Let the surface be cut by a plane passing through *OA, OB*. Then the section is, by hypothesis, a circle whose centre is *O*.

Thus *OA=OB;* which is contrary to the assumption. Therefore the surface cannot but be a sphere.

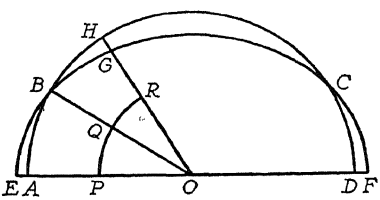
Proposition 2

The surface of any fluid at rest is the surface of a sphere whose centre is the same as that of the earth.

Suppose the surface of the fluid cut by a plane through *O*, the centre of the earth, in the curve *ABCD*.

ABCD shall be the circumference of a circle.

For, if not, some of the lines drawn from *O* to the curve will be unequal. Take one of them, *OB*, such that *OB* is greater than some of the lines from *O* to the curve and less than others. Draw a circle with *OB* as radius. Let it be *EBF*, which will therefore fall partly within and partly without the surface of the fluid.



Draw *OGH* making with *OB* an angle equal to the angle *EOB*, and meeting the surface in *H* and the circle in *G*. Draw also in the plane an arc of a circle *PQR* with centre *O* and within the fluid.

Then the parts of the fluid along *PQR* are uniform and continuous, and the part *PQ* is compressed by the part between it and *AB*, while the part *QR* is compressed by the part between *QR* and *BH*. Therefore the parts along *PQ, QR* will be unequally compressed, and the part which is compressed the less will be set in motion by that which is compressed the more.

Therefore there will not be rest; which is contrary to the hypothesis.

Hence the section of the surface will be the circumference of a circle whose centre is *O;* and so will all other sections by planes through *O*.

Therefore the surface is that of a sphere with centre *O*.

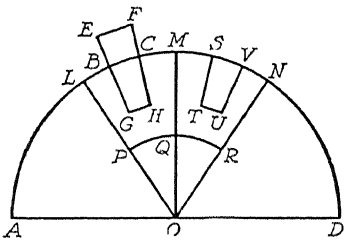
Proposition 3

Of solids those which, size for size, are of equal weight with a fluid will, if let down into the fluid, be immersed so that they do not project above the surface but do not sink lower.

If possible, let a certain solid *EFHG* of equal weight, volume for volume, with the fluid remain immersed in it so that part of it, *EBCF*, projects above the surface.

Draw through *O*, the centre of the earth, and through the solid a plane cutting the surface of the fluid in the circle *ABCD*.

Conceive a pyramid with vertex *O* and base a parallelogram at the surface of the fluid, such that it includes the immersed portion of the solid. Let this pyramid be cut by the plane of *ABCD* in *OL*, *OM*. Also let a sphere within the fluid and below *GH* be described with centre *O*, and let the plane of *ABCD* cut this sphere in *PQR*.



Conceive also another pyramid in the fluid with vertex *O*, continuous with the former pyramid and equal and similar to it. Let the pyramid so described be cut in *OM*, *ON* by the plane of *ABCD*.

Lastly, let *STUV* be a part of the fluid within the second pyramid equal and similar to the part *BGHC* of the solid, and let *SV* be at the surface of the fluid.

Then the pressures on *PQ*, *QR* are unequal, that on *PQ* being the greater. Hence the part at *QR* will be set in motion by that at *PQ*, and the fluid will not be at rest; which is contrary to the hypothesis.

Therefore the solid will not stand out above the surface.

Nor will it sink further, because all the parts of the fluid will be under the same pressure.

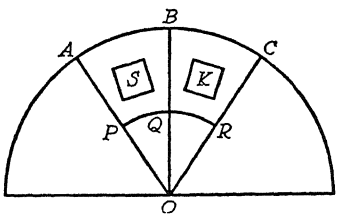
Proposition 4

A solid lighter than a fluid will, if immersed in it, not be completely submerged, but part of it will project above the surface.

In this case, after the manner of the previous proposition, we assume the solid, if possible, to be completely submerged and the fluid to be at rest in that position, and we conceive (1) a pyramid with its vertex at *O*, the centre of the earth, including the solid, (2) another pyramid continuous with the former and equal and similar to it, with the same vertex *O*, (3) a portion of the fluid within this latter pyramid equal to the immersed solid in the other pyramid, (4) a sphere with centre *O* whose surface is below the immersed solid and the part of the fluid in the second pyramid corresponding thereto. We suppose a plane to be drawn through

the centre *O* cutting the surface of the fluid in the circle *ABC*, the solid in *S*, the first pyramid in *OA*, *OB*, the second pyramid in *OB*, *OC*, the portion of the fluid in the second pyramid in *K*, and the inner sphere in *PQR*.

Then the pressures on the parts of the fluid at *PQ*, *QR* are unequal, since *S* is lighter than *K*. Hence there will not be rest; which is contrary to the hypothesis.

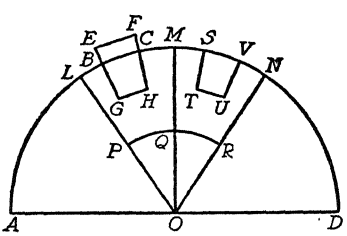


Therefore the solid *S* cannot, in a condition of rest, be completely submerged.

Proposition 5

Any solid lighter than a fluid will, if placed in the fluid, be so far immersed that the weight of the solid will be equal to the weight of the fluid displaced.

For let the solid be *EGHF*, and let *BGHC* be the portion of it immersed when the fluid is at rest. As in Prop. 3, conceive a pyramid with vertex *O* including the solid, and another pyramid with the same vertex continuous with the former and equal and similar to it. Suppose a portion of the fluid *STUV* at the base of the second pyramid to be equal and similar to the immersed portion of the solid; and let the construction be the same as in Prop. 3.

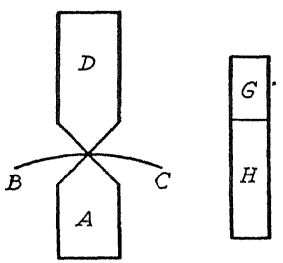


Then, since the pressure on the parts of the fluid at *PQ*, *QR* must be equal in order that the fluid may be at rest, it follows that the weight of the portion *STUV* of the fluid must be equal to the weight of the solid *EGHF*. And the former is equal to the weight of the fluid displaced by the immersed portion of the solid *BGHC*.

Proposition 6

If a solid lighter than a fluid be forcibly immersed in it, the solid will be driven upwards by a force equal to the difference between its weight and the weight of the fluid displaced.

For let A be completely immersed in the fluid, and let G represent the weight of A, and $(G+H)$ the weight of an equal volume of the fluid. Take a solid D, whose weight is H, and add it to A. Then the weight of $(A+D)$ is less than that of an equal volume of the fluid; and, if $(A+D)$ is immersed in the fluid, it will project so that its weight will be equal to the weight of the fluid displaced. But its weight is $(G+H)$.



Therefore the weight of the fluid displaced is $(G+H)$, and hence the volume of the fluid displaced is the volume of the solid A. There will accordingly be rest with A immersed and D projecting.

Thus the weight of D balances the upward force exerted by the fluid on A, and therefore the latter force is equal to H, which is the difference between the weight of A and the weight of the fluid which A displaces.

Proposition 7

A solid heavier than a fluid will, if placed in it, descend to the bottom of the fluid, and the solid will, when weighed in the fluid, be lighter than its true weight by the weight of the fluid displaced.

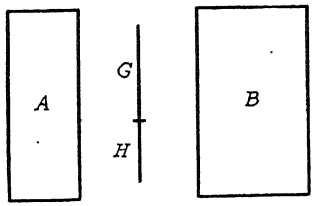
(1) The first part of the proposition is obvious, since the part of the fluid under the solid will be under greater pressure, and therefore the other parts will give way until the solid reaches the bottom.

(2) Let A be a solid heavier than the same volume of the fluid, and let $(G+H)$ represent its weight, while G represents the weight of the same volume of the fluid.

Take a solid B lighter than the same volume of the fluid, and such that the weight of B is G, while the weight of the same volume of the fluid is $(G+H)$.

Let A and B be now combined into one solid and immersed. Then, since $(A+B)$ will be of the same weight as the same volume of fluid,

both weights being equal to $(G+H)+G$, it follows that $(A+B)$ will remain stationary in the fluid.



Therefore the force which causes A by itself to sink must be equal to the upward force exerted by the fluid on B by itself. This latter is equal to the difference between $(G+H)$ and G [*Prop.* 6]. Hence A is depressed by a force equal to H, i.e. its weight in the fluid is H, or the difference between $(G+H)$ and G.

ON THE REVOLUTIONS OF THE HEAVENLY SPHERES

by

NIKOLAUS COPERNICUS

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NIKOLAUS COPERNICUS

1473–1543

NIKOLAUS COPERNICUS was born in Thorn, in East Prussia, in 1473, the son of a prosperous copper dealer who had married the daughter of a well-to-do merchant and landowner of that city. An orphan at ten, he came into the care of his mother's brother, a rising churchman named Lucas Watzelrode. When he was nineteen his uncle, now Bishop of Varmia, sent him to the University of Cracow; a few years later this uncle had him appointed a canon of Frauenberg cathedral. Thenceforth Copernicus held always a church office or two; he never experienced poverty.

At Cracow, Copernicus studied mathematics and astronomy, the use of astronomical instruments, and Aristotle. He was in Italy, apparently for the second time, in 1498, and he seems to have gone there to study medicine. He stayed for three years. During this time, probably, he learned Greek and developed a taste for humanistic studies. Certainly he studied astronomy at Bologna, and certainly he lectured on mathematics in Rome in 1500. At one time he registered at Bologna as a student of canon law. Subsequently he went to Padua to study medicine, then to Ferrara to study law, then back to Padua to study medicine again. In 1503 he returned to Varmia to live in close association with his uncle for almost a decade. A trained physician, he served as his uncle's secretary and companion, supervised the diet of the whole elaborately organized episcopal household, bought medical books for the bishop's library—many of which he annotated—and cared for the health of the bishop. He was constantly active in administrative matters as the agent of his uncle or of the cathedral chapter, constantly in contact in the great cities of East Prussia and Poland with powerful church and secular lords. Yet he found time for other activities. He translated the *Epistles of Simocatta*—a second-rate bit of late Greek

literature—into Latin and published his translation in 1509. It was the first original print of a Greek author in Poland. In the same year he observed a lunar eclipse, one of a large number on which during his long life he made extended notes. More important, he proceeded far enough with his theorizings and speculations to plan his book *De Revolutionibus Orbium Caelestium* (On the Revolutions of the Heavenly Spheres). Though thirty years lapsed before his book was printed, he may already have completed a draft of it when, at forty, he removed from Varmia to Frauenberg. He lived in Frauenberg almost continuously for the next thirty years, until his death in 1543.

During these long years, affairs of the church, of the cathedral chapter, of the secular world, intruded upon the scholar. In 1514 the Pope invited him to Rome to assist in the revision of the calendar. He refused, long afterward explaining that he could not accept because he had not then the accurate knowledge about the courses of the sun and moon which the revisionary task demanded. From 1515 to 1521 he was the administrator of Allenstein and Mehlsack, two tiny provinces in Ermland, and, after a war between the King of Poland and the Prussian Order—during which he defended the castle of Allenstein against the Prussians—he became administrator of all Ermland. In 1519, by invitation, he drew up a memorandum on the need and the means for stabilizing and improving the currency. This he presented to the Diets of Poland, Lithuania, and Prussia several times between 1519 and 1527. Unfortunately, though his ideas were sound, they were not adopted.

Meantime, the greatest tumult of the times, the revolt of Luther from the Church, apparently left Copernicus in remote Ermland quite unperturbed and untouched. A physician of some fame, an astronomer recognized in his own time, an able administrator in public affairs, an architect, a diplomat, a map maker, a warrior, a painter, an economist; indeed a man of almost universal abilities, he was yet a churchman but no theologian. While the storms of controversy roared over Europe, echoing even in Varmia, he continued quietly, persistently, to study the stars from his observatory higher than the cathedral roof in Frauenberg, to collect the data with which to support his theories, and to write and revise the book which eventually overthrew the accepted hypotheses of medieval astronomy.

The astronomical ideas of the Middle Ages all derived from Ptolemy, a second-century Alexandrian. He had left to his successors not only an admirable body of observations and computations, but also five hypotheses: (1) The World is a

sphere and revolves as a sphere; (2) The Earth is also a sphere; (3) The Earth is the center of the World; (4) In size, the Earth compared to the World is a mere point; (5) The Earth is motionless. No one had seriously doubted Ptolemy for fifteen hundred years; nor had anyone questioned his views, inherited from the Greek Hipparchus (160–125 B.C), that the planetary motions followed an intricate system of epicycles and eccentrics.

Copernicus particularly queried the fifth hypothesis. Others had done the same: Macrobius, John Scotus Erigena, Averroes, Maimonides, Nicolas of Cusa. But none had carried his queries very far. Copernicus convinced himself that this hypothesis was wholly untenable; then he discovered that number three similarly lacked validity.

In 1514, in a brief work called *Commentariolus,* Copernicus summed up his ideas in seven hypotheses: (1) There is no one center of all the celestial spheres; (2) The center of the Earth, though the center of gravity, is not the center of the World; (3) The planetary spheres revolve round the Sun as their center; (4) The distance of the Earth from the Sun is incommensurable with the dimensions of the firmament; (5) The Earth daily rotates on its axis; (6) The Earth performs more than one motion; (7) The motions of the Earth explain the apparent motions of the heavenly bodies. These propositions sharply modify those of Ptolemy. They are the essential propositions of the *De Revolutionibus.*

For full fifteen years after the composition of the brief *Commentariolus,* Copernicus busied himself in collecting data to substantiate his propositions. He came to believe that the older observations of astronomers were too inaccurate to be dependable, and he substituted for them his own more careful—though still faulty—observations and computations. Probably he constantly revised his book. When in 1539 a young Lutheran scholar from Wittenberg, Rheticus, sought out the famous astronomer in distant Frauenberg, the great work was apparently complete. Rheticus studied it enthusiastically, gave an account of it in a long formal letter to one of his scientific friends (printed as *Narratio Prima* in 1540), and two years later persuaded Copernicus to let him have the whole work printed. The first copy came into the old man's hands on the very day of his death in 1543.

Copernicus had originally planned his work in eight books; later he replanned it in six, of which the first is here translated. It presents the propositions of the *Commentariolus* together with the reasons, astronomical and geometrical, for accepting them. Book II is devoted to spherical astronomy; Book III, to the length of the year and the orbit of the earth;

Book IV, to the moon and its eclipses; Books V and VI, to the planetary motions. The work did not immediately win readers. Twenty years passed before there was a second printing (Basel, 1566), and another fifty before there was a third (1617). Even today it is not available in English. Yet in this long-neglected book Copernicus, almost singlehanded, overthrew the old geocentric theory and established the current heliocentric. Some of his "proofs" are now outmoded, chiefly because Copernicus had to rely upon observations and measurements made with the crudest of instruments. Some of his hypotheses later generations of astronomers have refused, notably the one concerning that motion of the earth which, according to him, explains the precession of the equinoxes. Nevertheless, this book, the lifework of one of the world's great men, is one of the world's greatest.

ON THE REVOLUTIONS OF THE HEAVENLY SPHERES

I. That the World is Spherical

FIRST, it must be recognized that the world is spherical. For the spherical is the form of all forms most perfect, having need of no articulation; and the spherical is the form of greatest volumetric capacity, best able to contain and circumscribe all else; and all the separated parts of the world—I mean the sun, the moon, and the stars—are observed to have spherical form; and all things tend to limit themselves under this form—as appears in drops of water and other liquids—whenever of themselves they tend to limit themselves. So no one may doubt that the spherical is the form of the world, the divine body.

II. That the Earth also is Spherical

SIMILARLY, the earth is spherical, all its sides resting upon its center. Of course, its perfect sphericity is not immediately seen because of the great height of the mountains and the great depth of the valleys. But these scarcely modify the total rotundity of the earth. Its sphericity is manifest. Indeed, for those who, from any part of the earth, journey towards the north, the pole of diurnal revolution little by little rises and the opposite pole declines, and many stars in the northern region seem never to set, whereas others in the southern regions seem never to rise. Thus Italy never sees Canopus, which is visible in Egypt. And Italy sees the last star of Fluvius, which our country, in a colder zone, knows naught of. Contrarily, for those who journey southward, these constellations rise whereas others, high for us, set. Nevertheless, the inclination of the poles has everywhere the same relation to any portion of the earth—which could not be true if the figure were not spherical. Hence it is clear that the earth is itself limited by poles and is consequently spherical. We may add that dwellers in the East do not see the eclipses of the sun and moon which chance to occur during the night, and that those of the West do not see those occurring by day; those between see these phenomena, some earlier and some later.

That the seas take a spherical form is perceived by navigators. For when land is still not discernible from a vessel's deck, it is from the masthead. And if, when a ship sails from land, a torch be fastened from the masthead, it appears to watchers on the land to go downward little by little until it entirely disappears, like a heavenly body setting. Yet it is certain that water, because of its fluidity, tends downward and does not rise above its container more than its convexity permits. That is why the land is so much the higher, why it rises from the ocean.

III. How the Land and Sea Form but One Globe

THE OCEAN which surrounds the land, pouring its waters every way, fills therewith the deepest depths. There is necessarily, therefore, less water in total than land—granted that both, because of their weight, tend toward the center; otherwise, the waters would cover the land. But for the safety of living creatures, the waters leave free some portions of land such as the numerous islands which are found here and there. As to the continent itself and the whole terrestrial world, is it not merely an island larger than the others?

It is unnecessary to heed those peripatetics who have affirmed that the quantity of water must be ten times that of the land because, as is notorious in the transmutation of elements, one part of land in liquefaction produces ten parts of water. Accepting that idea, they say that the land emerges just to a certain point because, possessing interior cavities, it is not in equilibrium with respect to gravity, and that the center of gravity is different from the center of volume. These men deceive themselves through their ignorance of geometry. They do not understand that even if there were seven times as much water as land, and if any part of the land remained dry, the land would have to withdraw wholly from the center of gravity, yielding place to the water as if it were the heavier element. For spheres are among themselves in the ratio of the cubes of their diameters. If, then, to seven parts of water the land were an eighth, its diameter could not be greater than the distance from the center to the circumference of the water. It is then still less possible that there should be ten times as much water as land. And that there is no difference between the center of gravity of the earth and its center of volume is proved by the fact that the convexity of the land which rises above the waters is not swollen in one smooth abscess; if it were, it would have thrust back the waters wholly and would not, in any manner, be subject to the inroads of interior seas and deep gulfs. Further, the greater the distance from the shore, the greater would be the ocean depths; and sailors departing from land would never encounter an island or a rock or any kind of land.

Now it is well known that between the Egyptian Sea and the Arabian Gulf, almost at the middle of the terrestrial world, the distance is scarcely fifteen stadii. Yet Ptolemy taught that the habitable earth extends to the

median circle; beyond that, he indicates unexplored land where moderns have identified Cathay and other vast areas reaching even to 60° of longitude. Thus the habitable land stretches through a greater longitude than is left for the ocean. And if thereto be added the islands discovered in our time under the Spanish and Portuguese princes, and especially all America —thus named by the ship's captain who discovered it—which from its dimensions (so far ill-known) appears to be a second continent, and numerous other islands hitherto unknown, one would not be greatly astonished to learn that there are antipodes and antichthones.

Indeed, geometric reasons force us to believe that America occupies a position diametrically opposite Gangean India. Hence, I think it clear that the land and the water alike tend toward a common center of gravity which is no other than the center of volume of the land, because it is the heavier. It is clear that the partly open portions of the land are filled with water, and that consequently, in comparison to the land, there is not much water even though, at the surface, there appears to be more water than land.

The land together with the water which encompasses it necessarily has the figure which its shadow reveals. Now, during eclipse, the shadow of the earth projected on the moon has the circumference of a perfect circle. In conclusion, then, the earth is not flat, even though Empedocles and Anaximenes thought so; nor is it drum-shaped, as Leucippus thought; nor is it boat-shaped, as Heraclitus thought; nor is it hollowed out in some other form, as Democritus believed; nor is it cylindrical, as Anaximander taught; no more is it infinitely extended downward, growing larger towards its base, as Xenophanes thought; but, as the philosophers thought, it is perfectly spherical.

IV. That the Movement of the Heavenly Bodies is Uniform, Perpetual, and Circular or Composed of Circular Movements

WE SHALL NOW remind ourselves that the motion of the heavenly bodies is circular. Indeed, for a sphere, the appropriate motion is rotation: by that very act, while it moves uniformly in itself, it expresses its form—that of the simplest of bodies in which can be distinguished neither beginning nor end, nor distinction between the one and the other.

Now because there are many spheres, there are varying motions. The most observable of these is the daily revolution which the Greeks called *nychthemeron,* that is, "the space of one day and one night." In that time, the whole of creation except the earth—so they believed—is borne from the east to the west. This motion has been accepted as the common measure for all other motions: we measure time itself usually by number of days. Then we see also other revolutions—some of which are retrograde, that is, going from west to east—notably those of the sun, the moon, and the five planets.

Thus the sun gives us the year and the moon the month, common

divisions of time; similarly, each of the five planets travels its own proper course. These motions, however, differ very strongly. First, they are not based on the same poles as the first revolution, but follow the slant of the zodiacal circle (the ecliptic). Then, in their individual circuits, they do not move in uniform fashion. The sun and the moon are discovered to be moving at one time more slowly, at another more rapidly. As for the five wandering stars, we see them sometimes even retrograding, and actually halting between their forward and backward motions. And though the sun ever travels along its route, these five wander in diverse fashions, now towards the south, now towards the north. This is, indeed, the reason for calling them wandering stars (planets). Further, sometimes they approach near to the earth—when they are said to be at the perigee—and at other times they proceed far from the earth—when they are said to be at the apogee. Nevertheless, it must be acknowledged that their paths are circular or composed of circles, for they execute their unequal motions in conformity with a certain law, and repeat the same motions periodically—a phenomenon impossible if their paths were not circular. Only the circle can bring back the past, as, for example, the sun by its motion composed of circular motions brings again to us the inequality of days and nights and of the four seasons.

Several different motions are recognized, for the heavenly bodies can not possibly be moved in an unequal fashion by a single sphere. Indeed, such inequality could occur only through the inconstancy of the moving power, which might conceivably arise from an external or an internal cause or by a modification in the revolving body. Now, since the intellect recoils with horror from these two suppositions, and since it would be unworthy to suppose such a thing in a creation constituted in the best way, it must be admitted that the equal movement of these bodies appears to us unequal either because the various spheres have not the same poles or because the earth is not the center of the circles round which they move. For us who from the earth view the movements of the heavenly bodies, they appear to be larger when they are near us than when they are more distant—an effect explained in optics. Thus the equal movements of the spheres may appear unequal motions in equal times to us, viewing them from different distances. This is the reason that I believe it first of all necessary for us to examine attentively the relation of the earth to the sky, so that, though we desire to study the highest things, we shall not be ignorant of those near at hand, and shall not, by similar error, attribute to heavenly bodies that which appertains to the earth.

V. Is a Circular Movement Suitable to the Earth?

IT HAS BEEN already demonstrated that the earth has the form of a globe, and I think it needful now to examine whether it follows a motion like to its form, and what is the place which it occupies in the universe. Without these bits of knowledge, it will not be possible to explain cer-

tain of the phenomena of the heavens. Certainly it is ordinarily so agreed among authors that the earth is at rest at the center of the world that they think it unreasonable and even ridiculous to maintain the contrary. If, however, we examine the question with great attention, it will emerge as not wholly solved, and not beneath inquiry. For all apparent local movement arises either from the motion of the thing observed, or from that of the observer, or from the simultaneous motions—of course unequal —of the two. If two bodies—I have in mind an observer and an object observed—move with equal motion, the motion is not perceived. Now it is from the earth that we observe the motions of the heavenly bodies. If, then, the earth did have some motion, we would observe it in the apparent motion of bodies external to the earth, as if they were swept along at an equal speed, but in an opposite sense; and such, in the first place, is the diurnal revolution. That seems, truly, to carry round the whole world except the earth and objects near it. If it were granted that the heavens have no motion but that the earth rotates from west to east, and if the result of such an assumed motion upon the apparent rising and setting of the sun were seriously examined, it would be found to be precisely as it now appears. And since the heavens embrace and contain all else, and are the common place of all things, it is not immediately clear why motion should be attributed rather to the containing body than to the body contained.

The Pythagoreans Heraclides and Ecphantus thought as much, and so, according to Cicero, did the Syracusan Nicetus. They conceived the earth to be turning at the center of the world. They considered that the stars "set" because the earth moved in front of them, and rose when the earth moved away. But if these views be accepted, there arises another problem no less important: What is the place of the earth? It is agreed by almost everyone that the earth is the center of the world. Yet if anyone were to deny this belief and should grant that the distance from the earth to the center of the world is by no means so great as to be comparable with the dimensions of the sphere of the fixed stars, yet still very great and, from the relations to the spheres of the suns and the other planets, quite obvious; if he should note that the motions of these later bodies appear irregular because they are controlled with relation to another center than the center of the earth; he might perhaps be able to offer an explanation not superficially absurd of the apparent irregularity in the motions of the heavenly bodies. For example: as the wandering stars are observed now nearer to the earth, now farther away, it necessarily follows that the earth is not the center of their circular paths. And it is not clear whether it is the earth which varies its distance from them or they which approach to and retreat from the earth.

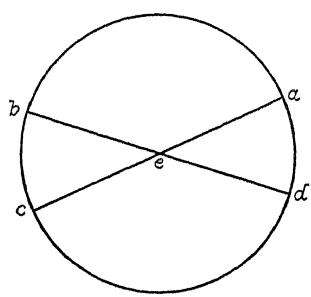
It would be scarcely surprising if someone were to attribute to the earth another motion besides the diurnal revolution. Indeed, Philolaus the Pythagorean, a remarkable mathematician, believed, they say, that the earth really moves circularly and at the same time executes several other motions. He considered the earth itself merely one of the stars. It was

to see him that Plato did not hesitate to travel to Italy, as those record who have narrated the life of Plato.

On the other hand, a number of philosophers have convinced themselves by geometric arguments that the earth is the center of the world. Indeed, only if it occupies the central position—being like a point in comparison to the immensity of the heavens—can it be, from that fact, motionless. For when the whole universe turns, its center remains still, and those things move slowest which are nearest to the center.

VI. Concerning the Immensity of the Heavens Compared to the Dimensions of the Earth

THAT the size of the earth, though huge, is yet not commensurable with that of the sky can be comprehended from what follows. The limiting circle (thus the Greek term *horizon* is interpreted) cuts the whole celes-



tial sphere into two halves, and it could not were the earth's size great compared to that of the sky or to its distance from the center of the world. As is well known, the circle which cuts a sphere into two halves is the greatest circle of the sphere which can be circumscribed upon the sphere's center. Let the circle *a b c d* be the horizon and let *e* be the earth from which we view the horizon, and itself the central point of the horizon which separates the visible from the non-visible stars. Now if, by means of a theodolite, a zodiacal chart, and a level placed at *e*, the beginning of Cancer is identified rising at *c*, at the same instant the beginning of Capricorn will be setting at *a*. But since the points *e, a,* and *c* are on a straight line running across the theodolite, clearly this line is the diameter of the zodiacal circle; for six signs of the zodiac circumscribe the visible stars, and the line center *c* is also the center of the horizon. Now, when a revolution has occurred and the beginning of Capricorn rises at *b*, then the beginning of Cancer is setting at *d*. Then *b e d* is a straight line and is a diameter of the zodiacal circle. But it has already been shown that *a e c* is similarly the diameter of the same circle. Clearly, the center of the circle is at the intersection of these two diameters. Thus, then, the horizon always cuts the circle of the zodiac, which is itself the greatest

possible circle of the sphere. And as, on a sphere, any circle which bisects a great circle is itself a great circle, it follows that the horizon is itself a great circle and that its center is the center of the ecliptic. Hence it is obvious that though the line passing across the earth's surface is different from the one passing through its center, yet because of the immensity of their lengths compared to the dimensions of the earth, they are like parallels which seem to form a single line. For because of the very hugeness of their length the distance between them becomes negligible in comparison—as is demonstrated in optics.

Thanks to this reasoning, it seems to be clear that the sky in comparison to the earth is immense, and may almost be considered infinite; and as reckoned by our senses, the earth compared to the sky is as a point to a body, or as the finite to the infinite. Precisely so much is demonstrated.

Now it does not follow from this concept that the earth must be motionless at the center of the world. Indeed, it would be more astonishing that the whole immense world should turn in twenty-four hours than that a little part of it, the earth, should. If it is claimed that a center is motionless and that those things nearest the center move most slowly, this does not prove that the earth remains motionless at the center of the world. It is easily said that the sky turns on unmoving poles, and that that which is nearest the poles is moved the least. Thus the Little Bear appears to us to move much more slowly than the Eagle or Sirius, for, close to the pole, it describes a very small circle; and since all these belong to one sphere, this sphere's motion being less near the pole of the axis does not allow that all its parts shall have motions equal the one to the other. The motion of the whole sweeps along the parts in their respective paths in equal time, but not through equal distances.

Observe now the consequence of the argument that the earth, being a part of the celestial sphere, participating in its nature and its motion, would be little moved because close to the center. It would be moved, it too, existing as a body not the geometric center of the sphere, and would describe in the same time circumferences like the celestial circles, but smaller. Now how false such a motion is, is clearer than day. Were it true, some part of the earth would be ever at high noon, and some other, ever at midnight. At no place would sunrise or sunset ever occur. For the motion of the whole and of the part would be one and inseparable.

Between things separated by a diversity of natures, the relation is wholly different, and such that those which travel a smaller circuit trace it more rapidly than those which travel a longer path. Saturn, for example, the most distant of the planets, moves round its circuit once in thirty years; whereas the moon, which is doubtless of all the planets the closest to the earth, accomplishes its whole journey in a month; and the earth itself turns in the space of a day and a night. Observe that the problem of the diurnal revolution recurs. So does that of the earth's place, not determined by what has preceded. For the earlier demonstration proves only the undefined immensity of the sky as compared to the size of the

earth. Yet how far that immensity extends is not at all clear. As with those tiny and indivisible bodies called atoms which, though they are not perceivable by themselves and do not when taken two or several together immediately form a visible body, yet may be multiplied until they join to form finally a great mass; just so it is with the place of the earth: although it is not itself at the center of the world, its distance from the center is not comparable with the immense dimensions of the sphere of the fixed stars.

VII. Why the Ancients Believed that the Earth is Motionless at the Middle of the World as its Center

For a variety of reasons the ancient philosophers asserted that the earth must be the center of the world. They adduced as a principal argument the matter of relative heaviness and lightness. Of the elements, earth is the heaviest; and all heavy objects move towards the earth, plunging towards its interior. Since the earth—towards which heavy things are borne from all sides and perpendicularly to the surface—is round, these heavy things would, if not restrained at the earth's surface, meet at the earth's center. For a straight line perpendicular to a surface tangential to a sphere leads to the sphere's center. Now objects which of themselves move towards a center seek to repose in the center. Surely, then, the earth must be in repose at its center. It receives in itself everything which falls, and must from its weight remain motionless.

These ancients sought to support the same belief by reasoning based on motion and its nature. Aristotle said that the motion of a single, simple body is simple; that of simple motions, one is circular, the other rectilinear; that of rectilinear motions, one is up and the other down. Consequently, every simple motion is directed toward the center—that is, down—or away from the center—that is, up—or around the center—that is, in a circle. To move downward—that is, toward the center—is proper only to the elements earth and water, regarded as the elements which have weight. To move up—that is, away from the center—is proper only to the elements air and fire, regarded as the elements which have lightness. These four elements are limited, therefore, to rectilinear motions; but the heavenly bodies turn round a center. Thus said Aristotle.

Ptolemy of Alexandria argued that if the earth turns, making even a daily revolution, the opposite of what has been said would occur. He shows that the motion which in twenty-four hours would turn the earth would be extremely violent and of an unsurpassable velocity. But things moved with a violent rotational motion are quite unlikely to cohere, but will rather disperse in fragments—unless they are held together by a superior force. And long ago, he says, a whirling earth would have been scattered beyond the sky itself (which is wholly ridiculous), and much more so all animate beings and other separate masses, none of which could have remained stable. Furthermore, were the earth turning, freely

falling bodies would never arrive perpendicularly at the points destined for them. And we would always see the clouds and other objects floating on the air moving towards the west.

VIII. A Refutation of the Arguments Quoted, and Their Insufficiency

For such reasons and others like them, the ancient philosophers affirmed that the earth stays always immobile at the center of the world, and that thereof there can be no doubt. But if anyone were to claim that the earth moves, he would surely say that this motion is natural and not violent. Now events occurring in conformity with nature produce results opposite to those caused by violence. Those things, indeed, to which are applied force and violence cannot long subsist and must needs soon be destroyed; but those which are in accord with nature exist in a proper way and in the best possible way.

Ptolemy therefore had no need to fear that the earth and all terrestrial beings would be destroyed by a rotation resulting from natural causes. Such a rotation wholly differs from one caused by art or by human enterprise. Why, indeed, on this head, did he not fear even more for the whole world, the motion of which would have to be as much more rapid as the heavens are greater in size than the earth? Have the heavens acquired their immensity because their motion, of an inexpressible magnitude, pulls them away from the earth? and would they fall if that motion ceased? Surely, if this reasoning were valid, the heavens would be infinite in extent. The more they are extended by the force of their motion, the more rapid would the motion become, for the distance to be traversed in twenty-four hours would be always increasing; and conversely, the immensity of the heavens would ever augment with the increase of the motion. Thus to very infinity the velocity would increase the magnitude of the motion, and the magnitude of the motion, the velocity. Then in agreement with this axiom of physics, "What is infinite cannot be traversed and cannot be moved," the heavens would necessarily halt.

It is alleged that beyond the heavens there is no body, no place, no void—nothing. Then there is only nothing into which the heavens could expand. Surely, too, it is astonishing that something should be stopped by nothing. And if the heavens are considered infinite and bounded only by an interior concavity, it is the more true that there is nothing beyond them, for everything must be within, whatever its dimensions may be. But from this argument, the heavens if infinite must be motionless; for the principal argument depended upon to show the world finite is its assumed motion.

Let us leave the philosophers to decide whether the world is finite or infinite. We are sure, in any event, that the earth between its poles is bounded by a spherical surface. Why then should we hesitate to attribute to it a motion properly according in nature with its form, rather than to disturb ourselves about the whole world, the limits of which we do not

and cannot know? Shall we not therefore admit that the daily revolution belongs in reality to the earth and its appearance only to the heavens? As Virgil's Aeneas said: "We depart from the port, and the cities and lands recede."

When a ship sails along without tossing, the sailors see all things exterior to the ship moving; they see, as it were, the image of their own motion; and they think themselves and all with them to be at rest. Possibly, in the same manner, we have believed the earth to be without motion and the whole world to move round it. What then about the clouds and all other bodies floating on the air, both those which fall and those which tend to rise? Very simply, we may think that not only the earth and the aqueous element which is a part of it move thus, but also the portion—not negligible—of the air and all its contents which have a relation to the earth. Either the air neighboring the earth, mixed with aqueous and earthy materials, shares in the nature of the earth, or the motion of the air is an acquired motion in which it participates because of the contiguity of the earth and its perpetual motion. As a contrary view, it is alleged—which is astounding—that the uppermost portion of the air shares in the motions of the heavens, and thus reveals those abruptly appearing stars which the Greeks called comets or "long-haired stars" (Lat., *pogoniae*), to the formation of which this uppermost air is assigned as place, and which, like other stars, rise and set. We can reply merely that if that part of the air, because of its great distance from the earth, is freed from the aforesaid terrestrial motion, the air nearest the earth and those things suspended in it will appear to be at rest until by the wind or some other force it is buffeted hither and yon. Is not a wind in the air like a current in the water?

As to things which by their nature rise or by their nature fall, we may affirm that in relation to the world their motions may be double and are generally composed of straight lines and circles. That things earthy in their nature are drawn downward by their weight is understandable, for indubitably the parts retain the nature of the whole. For no unlike reason are fiery things drawn upwards. Consider that terrestrial fire feeds on terrestrial matter; it is even said that flame is merely glowing smoke. Now the nature of fire is to distend that of which it takes possession, and it accomplishes this expansion with such force that it cannot in any manner or by any device be prevented from performing its work once it has shattered the imprisoning bonds. But an expanding motion is directed away from the center towards the circumference. Thus if any earthy portion be kindled, it must be borne away from the center, upwards. As has been said before, for a simple body the proper motion is simple (a fact verified particularly for circular motion) as long as that body retains its individuality and rests in its natural place. In that natural place, therefore, the motion is none other than circular, the motion which is self-contained, and likest to repose. Contrarily, motion in a straight line is the act of those things which move out of their proper places, which are forced from it, or for some other reason are outside it. Now nothing is

more repugnant to the form and order of the world than that something be out of its place. Therefore motion in a straight line is proper only to things which are not in order and which are not conforming to their nature—to things which are separated from their natural entities or have lost their essential individualities.

What is more, things which are impelled up or down, even neglecting their possible circular motion, do not execute a simple movement, uniform and equal. They conform consistently neither to their native lightness nor to the impulse of their weight. Those which fall execute first a slow motion which augments in velocity as they fall. Similarly, we see that terrestrial fire (and we see no other kind) as it rises simultaneously slows down, as if manifesting the force of the earthy material. Circular motion, however, always progresses in a uniform way, for it results from a constant cause. And again, things which move in straight lines soon put an end to their accelerated motion, because when they reach their destinations, they cease to be either light or heavy, and their motion stops. As, therefore, circular motion is proper to all complete, individual things, straight motion to partial things only, we may conclude that the circular motion stands toward the straight as the whole animal nature toward the sick member.

The fact that Aristotle divided simple motion into three kinds—away from the center, towards the center, and around the center—may be dismissed as merely an act of intellect. Just so, we distinguish the point, the line, and the surface, even though no one of them can exist without the others, and none of them without a body.

To all that precedes may be added that the state of immobility is usually considered more noble and more nearly divine than that of change and instability. For which is the state of rest more appropriate then, for the earth or for the world? It seems absurd to me to attribute motion to the containing and localizing rather than to the contained and localized —which is the earth.

Finally, since the planets clearly now approach and now recede from the earth, their movements being motions of single, self-contained bodies round a center, if the center of their revolutions is the center of the earth, their motion must be at one and the same time centripetal and centrifugal. Properly, one must conceive of the circular motion round a center in a more general fashion, and must be satisfied that the movement of each planet is related to its own true center.

For all the reasons given, then, motion for the earth is more probable than immobility; and especially is this true of the daily revolution, in as much as this motion is most proper for the earth. And I think that this discussion will suffice for the first part of the question.

IX. Whether Several Motions may be Attributed to the Earth; and of the Center of the World

SINCE then there are no reasons for our believing that the earth does not move, I think it proper now to question whether we may attribute to it several motions, whether it may not be thought of as one of the wandering stars (planets). That it is not the center of the motions of all the other heavenly bodies, their apparently unequal motions and varying distances from the earth demonstrate. For these variations cannot be explained for circular paths homocentric with the earth. But if there are several centers for these motions, it is not overbold to query whether the center of the world is the center of terrestrial gravity or some other center. For myself, I think that gravity is nothing other than a certain natural tendency given by the divine providence of the Architect of the World to the various parts so that they might assemble themselves into the one of which they are a part, coming together in the form of a globe. And it is credible that the same property belongs equally to the sun, to the moon, and to the other wandering stars. If it does, it might be thanks to its efficacy that although they travel their circuits in divers ways, they uniformly retain the roundness in which they appear.

If the earth does execute motions other than that around its center, they must be such, obviously, as will evidence themselves in many phenomena. Such a motion might be an annual progress round a circuit. If this annual motion be attributed to the earth, and if immobility be conceded to the sun, the rising and setting of the zodiacal signs and other fixed stars, thanks to which they are overhead in daytime as well as at night, will occur just as they do now. Then the progressions, halts, and retrogressions of the planets will be seen to be caused not by their motions, but by those of the earth which lends to the planets misleading appearances. Then, finally, it will have to be acknowledged that the sun occupies the center of the world.

These things both the law of the order in which they follow one after another and the harmony of the world combine to teach us, provided only that we look upon things themselves with, as it were, two eyes.

X. Of the Order of the Heavenly Bodies

I OBSERVE that no one questions that the heaven of the fixed stars is the highest of all which is visible. As to the order of the planets, we note that the ancient philosophers preferred to determine it according to the magnitudes of their respective revolutions. They reasoned that of bodies carried at equal speed, those which are more distant appear to be borne more slowly; this principle Euclid established in *The Optics*. They thought that as the moon completed its course in the briefest time, it

was borne round the smallest circle and was therefore closest to the earth. Saturn, which in the longest time travels the greatest circuit, they considered to be the highest or most distant. Nearer than Saturn they placed Jupiter; nearer than Jupiter, Mars. About Mercury and Venus, opinions varied; for these two never, like the others, proceed far from the sun. Some thinkers, therefore, like the Timaeus of Plato, placed them beyond the sun. Others, such as Ptolemy and a number of more recent scholars, place them this side of the sun. Alpetragius places Venus beyond the sun and Mercury on this side the sun.

Now those who agree with Plato in thinking that all the stars (otherwise dark bodies) shine only by light reflected from the sun argue that because the distance of these two planets from the sun is small, if they were below the sun they would be visible to us only in part, and never entirely round. Ordinarily, they would reflect the light they receive upwards—that is, towards the sun—as we see in the new and in the waning moon. They say, too, that sometimes the sun would necessarily be hidden from us by the interposition of these planets and that its light would for us be diminished in proportion to their size as they interposed. Since such a dimming never occurs, they conclude that these planets can in no fashion ever come this side the sun.

Those who place Mercury and Venus this side the sun base their argument on the vastness of the space which they discover between the sun and the moon. They have found that the greatest distance between the earth and the moon is sixty-four and one sixtieth times the distance from the center of the earth to its surface; and that the smallest distance between the earth and the sun is almost eighteen times the greatest distance between the earth and the moon. The distance between the earth and the sun is to the distance between the moon and the sun as 1160 is to 1096. In order, therefore, that so great a space need not be considered empty and void, and judging from the distances between the planetary orbits by which they calculate the depth of these orbits, they affirm that the space would be almost filled up if the distance between Mercury and the sun were less than that between the moon and the sun, and if the distance of Venus from the sun were less than that between Mercury and the sun, each of these distances being progressively smaller. Further, in this arrangement, the highest part of the orbit of Venus would approach very close to the sun. They calculate that between the aphelion and perihelion of Mercury there would be 177 times the distance between the earth and the moon, and that the remaining distance, 910 times that between the earth and the moon, would be almost filled by the apsidal dimensions of Venus. They also do not admit that there is any opacity in the stars, asserting that these shine either by their own light or by that of the sun impregnated in their entire bodies. These planets never darken because they only very rarely interpose between us and the sun; generally, they merely skirt the sun. And because these two are small bodies—Venus is larger than Mercury, but can yet hide not a hundredth part of the sun, according to Al Bategui the Aratonian who estimates the diameter of

the sun as ten times that of Venus—they believe that if either of them interposes between us and the sun, we would hardly see so small a speck in the sun's most resplendent light. Moreover, Averroes, in his paraphrase of Ptolemy, reports that he did see something blackish when he was observing the conjunction of Mercury and the sun which he had foretold by computations. Yet some persons judge that these two planets move wholly beyond the solar path.

How feeble and unsure is this reasoning becomes clear when we consider the fact that the least distance between the earth and the moon is, according to Ptolemy, thirty-eight times the distance from the earth's center to its surface (according to a better calculation, as will be shown later, more than forty-nine), yet we do not know that there is in all that space anything but air and, if it pleases us to think so, a certain fiery element. Furthermore, the diameter of the orbit of Venus, thanks to which it moves away from the sun by 45°, would have to be six times as great as the distance between the center of the earth and its perihelion, as will be demonstrated in the proper place. What do these reasoners maintain is contained in all that space, all the more that it would compass the earth, the air, the ether, the moon, and Mercury? So much must the huge epicycle of Venus embrace if that planet revolves round the motionless earth. How empty is Ptolemy's argument that the sun must occupy the mid-point between the planets moving away in all directions and those which do not depart is made clear by the moon which, itself moving away in every direction, exposes the falsity of the idea.

As to those who place Venus, then Mercury, on this side the sun, or arrange them in some other order, what reason can they allege that these do not effect the independent and different orbit of the sun, even as the other planets, unless the ratio of rapidity and slowness prevents any warping of the orbit?

It seems almost necessary to admit that the earth is not the center to which is referred the order of the stars and the orbits, even that there can be no reason for their order, and that one cannot know why the higher place belongs to Saturn rather than to Jupiter or some other planet. Perhaps that scheme is not despicable which was imagined by Martianus Capella (who wrote an encyclopedia) as well as by some other Latins. They held that Mercury and Venus revolve around the sun, which is at the center, and are unable to move further away from the sun than the convexities of their spheres permit. They thought that these two planets do not revolve round the earth, like the other planets, but have converse orbits. What can they wish to imply save that the center of these spheres is near the sun? If they are right, the sphere of Mercury is contained within that of Venus—which must be two or more times greater—and finds sufficient space within that amplitude.

Now if one should opportunely ascribe to that same center Saturn, Jupiter, and Mars—remembering that the dimensions of these spheres are such that within them they contain and embrace the earth also—he would not be far wrong; the canonic order of their motions declares it. Certainly,

these planets always approach nearest to the earth when they rise at evening; that is, when they are opposite the sun, the earth being between them and the sun. Contrarily, they are most distant when they set at even; that is, when they are hidden in the sunlight, when observably the sun is between them and the earth. This phenomenon shows adequately that the center of their circuits is associated with the sun and is, in fact, the same as that round which Mercury and Venus circle in their revolutions. If the spheres of these planets have all the same center, the space which remains between the convex side of the sphere of Venus and the concave side of the sphere of Mars must form another orb or sphere homocentric with those at its two surfaces. This sphere contains the earth with its companion the moon and with all that belongs within the lunar globe. For indeed we can in no fashion separate the moon from the earth to which it is, of heavenly bodies, incontestably the nearest, and the less need we to, in that the space left for it is sufficiently vast.

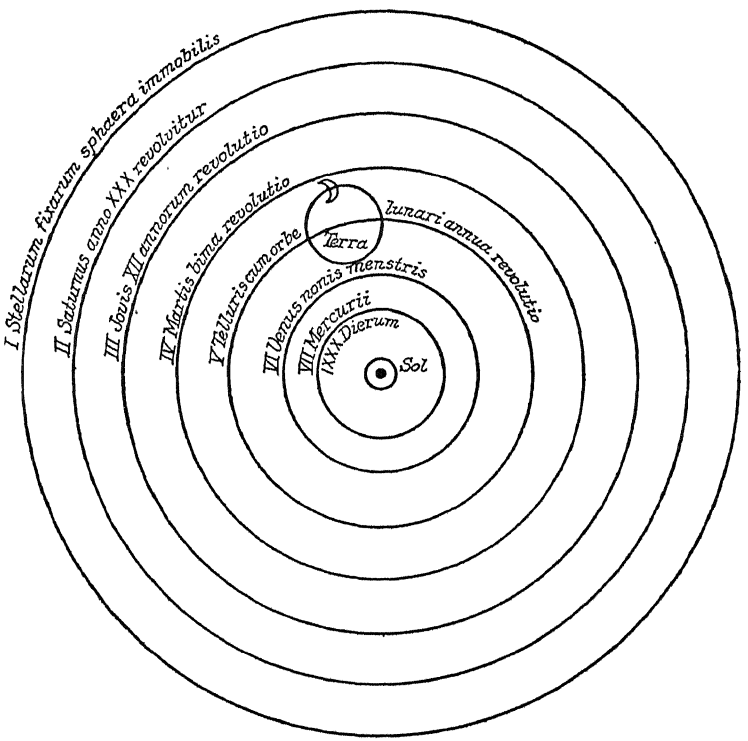
Therefore we need feel no shame in affirming that all which the moon's sphere embraces, even to the center of the earth, is drawn along by the motion of the greatest sphere, first as are the spheres of the other planets, in an annual revolution round the sun. Similarly, we dare assert that the sun is the center of the world, and that the sun remains motionless, all the motion which it appears to have being truly only an image of the earth's movement. And further, we may assert that the dimensions of the world are so vast that though the distance from the sun to the earth appears very large as compared with the size of the spheres of some planets, yet compared with the dimensions of the sphere of the fixed stars, it is as nothing.

All these assertions I find it easier to admit than to shatter reason by accepting the almost infinite number of spheres which those are forced to suppose who insist that the earth is the center of the world. It surely is better to conform to the wisdom of nature. Even as she dreads producing anything superfluous or useless, she often endows one causation with several effects.

The ideas here stated are difficult, even almost impossible, to accept; they are quite contrary to popular notions. Yet with the help of God, we will make everything as clear as day in what follows, at least for those who are not ignorant of mathematics. The first law being admitted—no one can propose one more suitable—that the size of the spheres is measured by the time of their revolutions, the order of the spheres immediately results therefrom, commencing with the highest, in the following way:

The first and highest of all the spheres is the sphere of the fixed stars. It encloses all the other spheres and is itself self-contained; it is immobile; it is certainly the portion of the universe with reference to which the movement and positions of all the other heavenly bodies must be considered. If some people are yet of the opinion that this sphere moves, we are of a contrary mind; and after deducing the motion of the earth, we shall show why we so conclude. Saturn, first of the planets, which accom-

plishes its revolution in thirty years, is nearest to the first sphere. Jupiter, making its revolution in twelve years, is next. Then comes Mars, revolving once in two years. The fourth place in the series is occupied by the sphere which contains the earth and the sphere of the moon, and which performs an annual revolution. The fifth place is that of Venus, revolving in nine months. Finally, the sixth place is occupied by Mercury, revolving in eighty days.



In the midst of all, the sun reposes, unmoving. Who, indeed, in this most beautiful temple would place the light-giver in any other part than that whence it can illumine all other parts? Not ineptly do some call the sun the lamp of the world, or the spirit of the world, or even the world's governor. Trismegistus calls it God visible; Sophocles' Electra, the all-seeing. Indeed, the sun, reposing as it were on a royal throne, controls the family of wandering stars which surrounds him. The earth will surely never be deprived of the ministry of the moon; as Aristotle says in *De Animalibus,* the earth and the moon enjoy the closest possible kinship. Meantime, the earth conceives by the sun and each year becomes great.

In this ordering there appears a wonderful symmetry in the world and

a precise relation between the motions and sizes of the spheres which no other arrangement offers. Herein the attentive observer can see why the progress and regress of Jupiter appear greater than Saturn's and less than Mars's; why also the progress and regress of Venus appear greater than Mercury's; why Saturn appears less often in reciprocation than Jupiter, and Mercury more often than Mars and Venus; why Saturn, Jupiter, and Mars are nearer to the earth when they rise at eventide than at the time of their occultation and apparition; why Mars when it becomes pernocturnal seems to equal Jupiter in size and can be distinguished from the latter only by its reddish color, and yet at other times is scarcely discoverable among stars of the second order unless by a careful observer working with a sextant. All these phenomena arise from the same cause: the movement of the earth.

That nothing similar can be discovered among the fixed stars proves their immense distance from us, a distance so immense as to render imperceptible to us even their apparent annual motion, the image of the earth's true motion. For every visible object or event there is a distance beyond which it cannot be seen, as is proven in optics. The glitter of the fixed stars' light shows that between the highest of the planetary spheres, Saturn's, and the sphere of the fixed stars, there is still an enormous space. It is by this glitter that the fixed stars are especially distinguishable from the planets; and it is proper that between the moving and the non-moving there should be a great difference. Thus perfect, truly, are the divine works of the best and supreme Architect.

XI. Demonstration of the Threefold Motion of the Earth

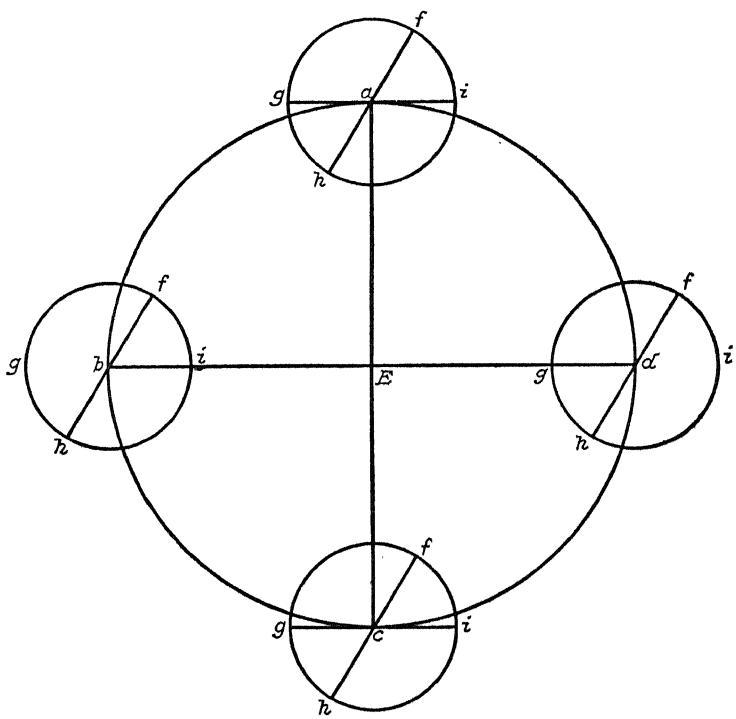
Since the numerous and important evidences from the planets support the hypothesis that the earth moves, we shall now expound that motion completely and shall show how far the motion hypothesized explains the phenomena. The motion is threefold. First there is the motion which the Greeks called *nychthemeron,* as we have said, which causes the sequence of day and night. This motion is executed from west to east—as it has been believed that the world moves in a contrary sense—and is a rotation of the earth on its axis. The motion traces the equinoctial circle which some, imitating the Greek expression, name the equidiurnal.

The second motion is the annual progress of the earth's center which, with all that is attached to it, travels round the sun on the circle of the zodiac. This motion is also from west to east, and it takes place, as we have said, between the spheres of Venus and Mars. Seemingly, it is the sun which executes a similar motion. Thus, when the center of the earth passes across Capricorn, Aquarius, and so forth, the sun seems to pass Cancer, Leo, and so on.

Next it must be recognized that the equator and the axis of the earth have a variable inclination with respect to the circle of the earth's path and the plane of this circle. Were the inclination fixed, there would be no

shifting inequality between days and nights; rather, at all times, there would exist the conditions of the equinox, or of the solstice, or of the shortest day, or of winter, or of summer, or of some other season. There must therefore be a third motion of the earth, varying the declination. This motion also is annual, but proceeds in a sense opposite to the motion of the center. Because these two motions are almost equal to one another but in opposite senses, the axis of the earth and the greatest of the parallel circles, the equator, face ever toward the same part of the world, as if they were motionless. Yet because of this motion of the earth, the sun appears to move obliquely on the ecliptic exactly as if the earth were the center of the world. The fact offers no difficulty provided one remembers that the distance from the earth to the sun is, compared to that from the sphere of the fixed stars, almost imperceptible.

There are matters better presented to the eye than expressed in words. I shall therefore trace the accompanying circle *a b c d* which may represent the annual motion of the earth's center in the plane of the ecliptic. In the center of the circle, *E* may represent the sun. I now cut the circle into four equal parts by means of the diameters *a E c* and *b E d* subtending equal arcs. Let us suppose that the point *a* is occupied by the beginning of Cancer, *b* by that of Libra, *c* by Capricorn, and *d* by Aries.



Let us also suppose that the center of the earth is, to begin with, at *a,* and let us trace the terrestrial equator *f g h i,* but not in the same plane, so that the diameter *g a i* may be common to both planes, that of the equator and that of the ecliptic. Then we shall trace similarly the diameter *f a h* at right angles to *g a i,* so that *f* shall be the limit of the greatest declination toward the south, and *h,* toward the north.

All these conditions being granted, the observer on the earth will see the sun—which is at the center *E*—in the position of the winter solstice, in Capricorn. This result arises from the greatest northern declination with respect to the sun. Conformably to the distance comprehended by the angle *E a h,* the inclination of the equator describes during the diurnal revolution the winter tropic.

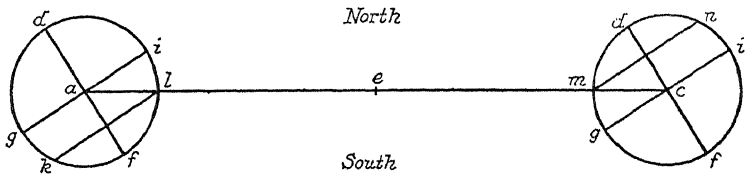
Let the center of the earth now advance until it reaches the point *b,* while at the same time *f,* the limit of the greatest declination, advances in a contrary sense. Each will now have described a quarter circle. During this time, because of the equality of the two motions, the angle *E a i* will always remain equal to the angle *a E b,* and the diameters *f a h* and *g a i* will remain parallel to the diameters *f b h* and *g b i,* as will the equator. Because of the immensity of the sky, often mentioned before, they will appear the same.

Now, from the point *b*—the beginning of Libra—*E* will appear to be in Aries, and the common sections of the two circles will coincide in a single line, *g b i E.* In relation to this line, all declination is lateral, and the daily revolution reveals no declination. The sun appears to be in the position right for the spring equinox.

Let the earth continue its journey under the conditions specified until, having traveled half its route, it has reached *c.* The sun is now apparently entering Cancer. Since the southern declination of the equator, *f,* is now turned toward the sun, it will during the diurnal revolution move along the summer tropic, as measured by the angle *E c f.*

When *f* has moved through the third quadrant of the circle, the common section *g i* will coincide again with the line *E d,* and the sun will be observed in Libra; that is, the sun is at the position for the autumn equinox. Then, the same motion continuing, and *h f* turning little by little again toward the sun, there will result the original situation, that with which we started.

Another explanation. In the accompanying diagram, let *a e c* be the diameter of the ecliptic and represent the line common to the circle *a b c* and the circle of the ecliptic in the preceding diagram; this diagram is at right angles to the preceding one. In this new diagram, at *a* and at *c,* that is, in Cancer and in Capricorn, let us draw *d g f i,* which will represent a meridian of the earth, and *d f,* which will represent its axis. The north pole is at *d;* the south at *f;* and *g i* the equatorial diameter. As before, the sun is at *e.* When *f* turns toward the sun and the inclination of the equator is north by the angle *i a e,* as the earth rotates on its axis, the chord *c l* at the distance *i l* from the equator will describe the southern circle parallel to the equator. This circle appears in the sun as the tropic of Capricorn.



To speak more exactly, as the earth rotates on its axis, the line *a e* describes a conic section of which the apex is the center of the earth, and of which the base is parallel to the equator.

In the opposite sign of the zodiac, at *c*, precisely the same things will be true—but in the inverse sense.

It is clear, therefore, how the two mutually opposed motions—I mean those of the center and of the inclination—compel the axis of the earth to remain ever at the same inclination and in the same position. Equally clear is it that these motions appear to be motions of the sun.

We have said that the annual revolution of the center and of the inclination are almost equal. Were they precisely equal, the equinoctial and solstitial points, and the obliquity of the ecliptic with respect to the fixed stars, ought never to change. They are not precisely equal, and hence a change occurs, but so small that it is revealed only over a long period of time. For example, from Ptolemy's time to ours, the solstitial and equinoctial points have executed a precession of twenty-one degrees. From this observation, some men have argued that the sphere of the fixed stars also moves. Some talk of a ninth sphere; and as that does not suffice to explain everything, the moderns now add a tenth. They still do not attain their end. But using the movements of the earth as a principle and as a hypothesis, we hope to explain even more phenomena. If anyone maintains that the motions of the sun and of the moon can be explained on the hypothesis that the earth is immobile, the explanation offered does not accord with the motions of the other planets. Probably it was for this reason or some other similar reason—and not for the reasons alleged and refuted by Aristotle—that Philolaus admitted that it is the earth which moves. According to some authorities, Aristarchus held the same opinion.

These are matters which can, indeed, be understood only by a penetrating spirit and after long study. Knowledge of them was consequently rare among the philosophers. True, the number of those who studied the motions of the stars was very small; and it did not include Plato. And even if these matters were understood by Philolaus and some other Pythagoreans, it is not strange that their knowledge did not survive among their successors. For the Pythagoreans were not accustomed to entrust their secrets to books, or to initiate the whole world into the mysteries of philosophy. They rather confided only in their friends and kinsmen, passing their secrets on only, as it were, from hand to hand. Of this fact, the letter of Lysis to Hipparchus gives evidence. With a reference to its sentiments on secrecy, worthy to be remembered, it pleases me to end this first book.

DIALOGUES CONCERNING TWO NEW SCIENCES

by

GALILEO

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Dialogues Concerning Two New Sciences

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GALILEO GALILEI

1564–1642

VINCENZIO GALILEI, a poor nobleman of Florence, early recognized the talents of his son Galileo. An able musician and a good mathematician, he sent the boy, born in Pisa in 1564, first to study Greek, Latin, and logic at the monastery of Vallombrosa, near Florence, and, in 1581, to the University of Pisa to study medicine. The boy had already shown aptitude in music and in painting, and a small talent in literature—eventually visible in an inconsiderable comedy, in a few minor poems, in some critical remarks on Ariosto, and in a voluminous and eloquent correspondence. Vincenzio had, however, decided on medicine as his son's profession. He had, indeed, allowed the boy training in the recognized arts; but he had kept him wholly from any study in mathematics.

Quite by accident, Galileo overheard at the university a lecture on geometry. His interest flared so high that he persuaded his father to let him have mathematical instruction. From this time on, though he stayed at the university until he was twenty-one, he read medicine no more. Instead he devoted himself to mathematics and mechanics, and continued the study of these sciences for the remainder of a long, fruitful life. In 1585, apparently for want of funds, Vincenzio withdrew his son from the University of Pisa. The young man returned to Florence and secured an appointment as lecturer in mathematics at the Academy there.

Already, during his years in Pisa, Galileo had made the first of those observations in mechanics which were to bring him fame. He had watched the swaying of a lamp suspended on a long chain in the Cathedral of Pisa sharply enough to discover that whatever the range of the oscillations, they were executed in the same time. He conducted some verifying experiments, discovered the isochronism of the pendulum, and, perhaps because he had somewhat studied medicine, applied

the newly discovered principle to the timing of the human pulse. In 1586 he published an account of a hydrostatic balance which he had invented, and his name began to be widely known in Italy. Two years later he wrote a treatise on the center of gravity in solids. As a result, he was recalled to Pisa as a professor of mathematics in the university. The young medical matriculant of seven years before had wholly shifted his ground. Yet old Vincenzio was not denied matter for pride in his son.

Once more in Pisa, Galileo made the observations in mechanics which, confirmed according to his usual method by experiments, led him to the discovery that bodies of differing masses fall with equal acceleration from rest, and to the discovery that the path of a projectile is a parabola. That he demonstrated his discoveries by dropping and tossing objects from the top of the Leaning Tower of Pisa is an often repeated story which has no foundation in fact. It is a fiction of his later biographers, supported by imaginative illustrators of "great moments in the history of science." But if not in the Leaning Tower, at least in Pisa, he made these discoveries, fundamental to the whole theory of dynamics.

Galileo's discovery about velocity and acceleration in free fall contradicted the views his contemporaries held, views for which they thought they had the authority of Aristotle. They attacked Galileo, pooh-poohed his theories, and provoked him to sarcastic replies. Thus he entered into the first of the controversies which enlivened his public life. He also earned such unpopularity that he had to resign his university appointment and return to Florence. He lived in that city quietly for a year, and was then called to Padua to become professor of mathematics in the university there. He continued to live in Padua for the next twenty years. Even when, in 1610, he left Padua permanently, he retained his professorship in the university, for it had been granted for life. The stipend from this appointment, together with the income from some sinecures which came to him as the rewards of increasing age and fame, enabled him to continue his studies, his theorizing, his experiments—and his controversies—without recourse to any activity of which the end would have been merely economic independence.

In 1609, during a visit to Venice, Galileo heard a rumor that a telescope had been invented by a lens maker in the Low Countries. He returned to Padua, busied himself with experiment, and in a few days hastened to Venice to present to the Doge the first telescope known to Venice. It magnified three diameters. Galileo patiently learned the technique of grinding and polishing lenses; he experimented with arrangements of

lenses. Eventually he constructed an instrument which magnified thirty-three diameters. Meantime he had begun a series of astronomical observations with his telescopes. He observed the mountains of the moon, the satellites of Jupiter, sun spots, the constituent stars of the Milky Way. He manufactured telescopes in sufficient numbers to supply a great part of Europe, and so firmly was his name attached to the device that to this day the telescope he used—of which the modern opera glass is a type—though he was not its real inventor, is called the Galilean telescope.

The astronomical observations which Galileo made contributed a great deal to the stock of information of astronomers. Most particularly, it provided evidence for the validity of the Copernican theories—as against the Ptolemaic—explaining the motion of heavenly bodies. Galileo had accepted the Copernican ideas as early as 1597, but a fear of ridicule had restrained him from a public avowal of his opinion. In 1613, after he had demonstrated his telescope to an acclaiming public in Rome, he published *Letters on the Solar Spots* in which he argued for the Copernican views. His great reputation provoked ecclesiastical authorities to examine this work, and they found that the new views ran counter to conventional interpretation of Biblical texts. Immediately a controversy of large dimensions arose. Galileo threw himself into it avidly. He lectured and demonstrated and wrote. He sought out Biblical texts to support his position. The consequence was that in 1616 the theologians of the Holy Office decided that the Copernican theories were heretical, and Pope Paul V admonished Galileo not "to hold, teach, or defend" the condemned doctrine. He was, in short, advised to avoid theology and to restrict himself to physical reasoning; and he promised to heed the advice.

Galileo retired once more to Florence, to seven years of studious quiet. Then he published a work on comets, dedicated to Urban VIII, the new pope. The intellectual atmosphere seemed less suffocating than some years earlier. Meantime he had become ever more convinced of the truth of the Copernican theories. He now, in the freer air, reweighed all the arguments, discussed them with friends, and finally, in 1630, completed *Dialogo dei due massimi sistemi del mondo* (Dialogue Concerning the Two Chief Systems of the World), a work which really demolished the Ptolemaic doctrine and established that of Copernicus. When he published the work in 1632, Europe applauded. But the Inquisition at Rome had not forgotten that Galileo had promised sixteen years before not to "hold, teach, or defend" the forbidden doctrine. Sale of the book was banned; Galileo was cited to Rome for trial.

Eventually he did stand trial in Rome, recanted, and was condemned to recite the seven penitential psalms once a week for three years.

There is a tale that Galileo, rising from the kneeling position in which, before the trial officers, he had agreed that the earth stands stationary while the sun moves round it, stamped on the earth and muttered, "It does move, anyway." The story is pure fiction. But whatever the words he muttered, if he muttered any, it is reasonable to believe that he privately held to his published ideas. For his intellectual vigor had not declined. He had yet eight years to live, and even after he had become blind in 1637, he continued his speculations on physical subjects. Indeed, he was dictating to his pupils, Torricelli and Viviani, his latest ideas on impact when he was seized by the slow fever of which, in 1642, he died.

The later years of Galileo's life were notable not for new discoveries, but for the composition and publication of his *Dialoghi delle nuove scienze* (Dialogues Concerning New Sciences). In these he recounted the bulk of his experimental and theoretical work, and literally laid the foundations for the science of mechanics. Though some isolated notions had been grasped by his predecessors, Galileo first clearly understood and presented the idea of force as a mechanical agent. He further showed how a combination of experiment with calculation, the perpetual comparison of results, the translation of the concrete into the abstract, provide a method for investigating natural laws. Of such laws he stated many, and his work implies the knowledge and understanding of others. The science of mechanics rests upon the three Laws of Motion which Newton, not many years after Galileo's death, enunciated in their final form. Galileo never stated these Laws; yet his work suggests that he was aware of the principles they codify. In the *Dialogues,* his last work, he explored the territory which Newton was later to survey and measure. And these *Dialogues,* better than any notes on Galileo, illustrate his methods and reveal his discoveries.

(In the *Dialogues,* Galileo presents his own arguments as the words of Salviati. He also refers occasionally to himself as "Author" or as "Academician.")

DIALOGUES CONCERNING TWO NEW SCIENCES

FIRST DAY

Interlocutors: Salviati, Sagredo and Simplicio

SALVIATI. The constant activity which you Venetians display in your famous arsenal suggests to the studious mind a large field for investigation, especially that part of the work which involves mechanics; for in this department all types of instruments and machines are constantly being constructed by many artisans, among whom there must be some who, partly by inherited experience and partly by their own observations, have become highly expert and clever in explanation.

SAGREDO. You are quite right. Indeed, I myself, being curious by nature, frequently visit this place for the mere pleasure of observing the work of those who, on account of their superiority over other artisans, we call "first rank men." Conference with them has often helped me in the investigation of certain effects including not only those which are striking, but also those which are recondite and almost incredible. At times also I have been put to confusion and driven to despair of ever explaining something for which I could not account, but which my senses told me to be true. And notwithstanding the fact that what the old man told us a little while ago is proverbial and commonly accepted, yet it seemed to me altogether false, like many another saying which is current among the ignorant; for I think they introduce these expressions in order to give the appearance of knowing something about matters which they do not understand.

SALVIATI. You refer, perhaps, to that last remark of his when we asked the reason why they employed stocks, scaffolding and bracing of larger dimensions for launching a big vessel than they do for a small one; and he answered that they did this in order to avoid the danger of the ship parting under its own heavy weight, a danger to which small boats are not subject?

SAGREDO. Yes, that is what I mean; and I refer especially to his last assertion which I have always regarded as a false, though current, opinion; namely, that in speaking of these and other similar machines one cannot argue from the small to the large, because many devices which succeed on

a small scale do not work on a large scale. Now, since mechanics has its foundation in geometry, where mere size cuts no figure, I do not see that the properties of circles, triangles, cylinders, cones and other solid figures will change with their size. If, therefore, a large machine be constructed in such a way that its parts bear to one another the same ratio as in a smaller one, and if the smaller is sufficiently strong for the purpose for which it was designed, I do not see why the larger also should not be able to withstand any severe and destructive tests to which it may be subjected.

SALVIATI. The common opinion is here absolutely wrong. Indeed, it is so far wrong that precisely the opposite is true, namely, that many machines can be constructed even more perfectly on a large scale than on a small; thus, for instance, a clock which indicates and strikes the hour can be made more accurate on a large scale than on a small. There are some intelligent people who maintain this same opinion, but on more reasonable grounds, when they cut loose from geometry and argue that the better performance of the large machine is owing to the imperfections and variations of the material. Here I trust you will not charge me with arrogance if I say that imperfections in the material, even those which are great enough to invalidate the clearest mathematical proof, are not sufficient to explain the deviations observed between machines in the concrete and in the abstract. Yet I shall say it and will affirm that, even if the imperfections did not exist and matter were absolutely perfect, unalterable and free from all accidental variations, still the mere fact that it is matter makes the larger machine, built of the same material and in the same proportion as the smaller, correspond with exactness to the smaller in every respect except that it will not be so strong or so resistant against violent treatment; the larger the machine, the greater its weakness. Since I assume matter to be unchangeable and always the same, it is clear that we are no less able to treat this constant and invariable property in a rigid manner than if it belonged to simple and pure mathematics. Therefore, Sagredo, you would do well to change the opinion which you, and perhaps also many other students of mechanics, have entertained concerning the ability of machines and structures to resist external disturbances, thinking that when they are built of the same material and maintain the same ratio between parts, they are able equally, or rather proportionally, to resist or yield to such external disturbances and blows. For we can demonstrate by geometry that the large machine is not proportionately stronger than the small. Finally, we may say that, for every machine and structure, whether artificial or natural, there is set a necessary limit beyond which neither art nor nature can pass; it is here understood, of course, that the material is the same and the proportion preserved.

SAGREDO. My brain already reels. My mind, like a cloud momentarily illuminated by a lightning flash, is for an instant filled with an unusual light, which now beckons to me and which now suddenly mingles and obscures strange, crude ideas. From what you have said it appears to me impossible to build two similar structures of the same material, but of different sizes and have them proportionately strong; and if this were so,

it would not be possible to find two single poles made of the same wood which shall be alike in strength and resistance but unlike in size.

SALVIATI. So it is, Sagredo. And to make sure that we understand each other, I say that if we take a wooden rod of a certain length and size, fitted, say, into a wall at right angles, i. e., parallel to the horizon, it may be reduced to such a length that it will just support itself; so that if a hair's breadth be added to its length it will break under its own weight and will be the only rod of the kind in the world. Thus if, for instance, its length be a hundred times its breadth, you will not be able to find another rod whose length is also a hundred times its breadth and which, like the former, is just able to sustain its own weight and no more: all the larger ones will break while all the shorter ones will be strong enough to support something more than their own weight. And this which I have said about the ability to support itself must be understood to apply also to other tests; so that if a piece of scantling will carry the weight of ten similar to itself, a beam having the same proportions will not be able to support ten similar beams.

Please observe, gentlemen, how facts which at first seem improbable will, even on scant explanation, drop the cloak which has hidden them and stand forth in naked and simple beauty. Who does not know that a horse falling from a height of three or four cubits will break his bones, while a dog falling from the same height or a cat from a height of eight or ten cubits will suffer no injury? Equally harmless would be the fall of a grasshopper from a tower or the fall of an ant from the distance of the moon. Do not children fall with impunity from heights which would cost their elders a broken leg or perhaps a fractured skull? And just as smaller animals are proportionately stronger and more robust than the larger, so also smaller plants are able to stand up better than larger. I am certain you both know that an oak two hundred cubits high would not be able to sustain its own branches if they were distributed as in a tree of ordinary size; and that nature cannot produce a horse as large as twenty ordinary horses or a giant ten times taller than an ordinary man unless by miracle or by greatly altering the proportions of his limbs and especially of his bones, which would have to be considerably enlarged over the ordinary. Likewise the current belief that, in the case of artificial machines the very large and the small are equally feasible and lasting is a manifest error. Thus, for example, a small obelisk or column or other solid figure can certainly be laid down or set up without danger of breaking, while the very large ones will go to pieces under the slightest provocation, and that purely on account of their own weight. And here I must relate a circumstance which is worthy of your attention as indeed are all events which happen contrary to expectation, especially when a precautionary measure turns out to be a cause of disaster. A large marble column was laid out so that its two ends rested each upon a piece of beam; a little later it occurred to a mechanic that, in order to be doubly sure of its not breaking in the middle by its own weight, it would be wise to lay a third support midway; this seemed to all an excellent idea; but the sequel showed that

it was quite the opposite, for not many months passed before the column was found cracked and broken exactly above the new middle support.

SIMPLICIO. A very remarkable and thoroughly unexpected accident, especially if caused by placing that new support in the middle.

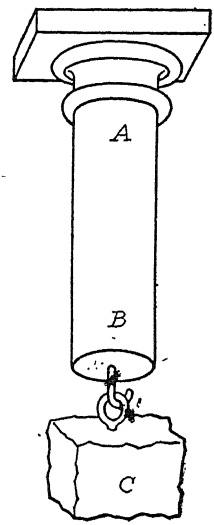
SALVIATI. Surely this is the explanation, and the moment the cause is known our surprise vanishes; for when the two pieces of the column were placed on level ground it was observed that one of the end beams had, after a long while, become decayed and sunken, but that the middle one remained hard and strong, thus causing one half of the column to project in the air without any support. Under these circumstances the body therefore behaved differently from what it would have done if supported only upon the first beams; because no matter how much they might have sunken the column would have gone with them. This is an accident which could not possibly have happened to a small column, even though made of the same stone and having a length corresponding to its thickness, i. e., preserving the ratio between thickness and length found in the large pillar.

SAGREDO. I am quite convinced of the facts of the case, but I do not understand why the strength and resistance are not multiplied in the same proportion as the material; and I am the more puzzled because, on the contrary, I have noticed in other cases that the strength and resistance against breaking increase in a larger ratio than the amount of material. Thus, for instance, if two nails be driven into a wall, the one which is twice as big as the other will support not only twice as much weight as the other, but three or four times as much.

SALVIATI. Indeed you will not be far wrong if you say eight times as much; nor does this phenomenon contradict the other even though in appearance they seem so different.

SAGREDO. Will you not then, Salviati, remove these difficulties and clear away these obscurities if possible: for I imagine that this problem of resistance opens up a field of beautiful and useful ideas; and if you are pleased to make this the subject of today's discourse you will place Simplicio and me under many obligations.

SALVIATI. I am at your service if only I can call to mind what I learned from our Academician [Galileo] who had thought much upon this subject and according to his custom had demonstrated everything by geometrical methods so that one might fairly call this a new science. For, although some of his conclusions had been reached by others, first of all by Aristotle, these are not the most beautiful and, what is more important, they had not been proven in a rigid manner from fundamental principles. Now, since I wish to convince you by demonstrative reasoning rather than to persuade you by mere probabilities, I shall suppose that you are familiar with present-day mechanics so far as it is needed in our discussion. First of all it is necessary to consider what happens when a piece of wood or any other solid which coheres firmly is broken; for this is the fundamental fact, involving the first and simple principle which we must take for granted as well known.



To grasp this more clearly, imagine a cylinder or prism, AB, made of wood or other solid coherent material. Fasten the upper end, A, so that the cylinder hangs vertically. To the lower end, B, attach the weight C. It is clear that however great they may be, the tenacity and coherence between the parts of this solid, so long as they are not infinite, can be overcome by the pull of the weight C, a weight which can be increased indefinitely until finally the solid breaks like a rope. And as in the case of the rope whose strength we know to be derived from a multitude of hemp threads which compose it, so in the case of the wood, we observe its fibres and filaments run lengthwise and render it much stronger than a hemp rope of the same thickness. But in the case of a stone or metallic cylinder where the coherence seems to be still greater the cement which holds the parts together must be something other than filaments and fibres; and yet even this can be broken by a strong pull.

SIMPLICIO. If this matter be as you say I can well understand that the fibres of the wood, being as long as the piece of wood itself, render it strong and resistant against large forces tending to break it. But how can one make a rope one hundred cubits long out of hempen fibres which are not more than two or three cubits long, and still give it so much strength? Besides, I should be glad to hear your opinion as to the manner in which the parts of metal, stone, and other materials not showing a filamentous structure are put together; for, if I mistake not, they exhibit even greater tenacity.

SALVIATI. To solve the problems which you raise it will be necessary to make a digression into subjects which have little bearing upon our present purpose.

SAGREDO. But if, by digressions, we can reach new truth, what harm is there in making one now, so that we may not lose this knowledge, remembering that such an opportunity, once omitted, may not return; remembering also that we are not tied down to a fixed and brief method but that we meet solely for our own entertainment? Indeed, who knows but that we may thus frequently discover something more interesting and beautiful than the solution originally sought? I beg of you, therefore, to grant the request of Simplicio, which is also mine; for I am no less curious and desirous than he to learn what is the binding material which holds together the parts of solids so that they can scarcely be separated. This information is also needed to understand the coherence of the parts of fibres themselves of which some solids are built up.

SALVIATI. I am at your service, since you desire it. The first question is, How are fibres, each not more than two or three cubits in length, so

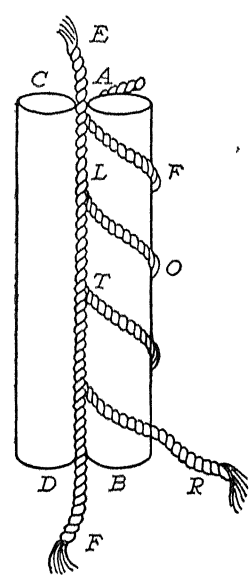
tightly bound together in the case of a rope one hundred cubits long that great force is required to break it?

Now tell me, Simplicio, can you not hold a hempen fibre so tightly between your fingers that I, pulling by the other end, would break it before drawing it away from you? Certainly you can. And now when the fibres of hemp are held not only at the ends, but are grasped by the surrounding medium throughout their entire length is it not manifestly more difficult to tear them loose from what holds them than to break them? But in the case of the rope the very act of twisting causes the threads to bind one another in such a way that when the rope is stretched with a great force the fibres break rather than separate from each other.

At the point where a rope parts the fibres are, as everyone knows, very short, nothing like a cubit long, as they would be if the parting of the rope occurred, not by the breaking of the filaments, but by their slipping one over the other.

SAGREDO. In confirmation of this it may be remarked that ropes sometimes break not by a lengthwise pull but by excessive twisting. This, it seems to me, is a conclusive argument because the threads bind one another so tightly that the compressing fibres do not permit those which are compressed to lengthen the spirals even that little bit by which it is necessary for them to lengthen in order to surround the rope which, on twisting, grows shorter and thicker.

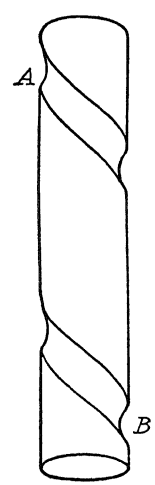
SALVIATI. You are quite right. Now see how one fact suggests another. The thread held between the fingers does not yield to one who wishes to draw it away even when pulled with considerable force, but resists because it is held back by a double compression, seeing that the upper finger presses against the lower as hard as the lower against the upper. Now, if we could retain only one of these pressures there is no doubt that only half the original resistance would remain; but since we are not able, by lifting, say, the upper finger, to remove one of these pressures without also removing the other, it becomes necessary to preserve one of them by means of a new device which causes the thread to press itself against the finger or against some other solid body upon which it rests; and thus it is brought about that the very force which pulls it in order to snatch it away compresses it more and more as the pull increases. This is accomplished by wrapping the thread around the solid in the manner of a spiral; and will be better understood by means of a figure. Let AB and CD be two cylinders between which is stretched the thread EF: and for the sake of greater clearness we will imagine it to be a small cord. If these



two cylinders be pressed strongly together, the cord EF, when drawn by the end F, will undoubtedly stand a considerable pull before it slips between the two compressing solids. But if we remove one of these cylinders the cord, though remaining in contact with the other, will not thereby be prevented from slipping freely. On the other hand, if one holds the cord loosely against the top of the cylinder A, winds it in the spiral form AFLOTR, and then pulls it by the end R, it is evident that the cord will begin to bind the cylinder; the greater the number of spirals the more tightly will the cord be pressed against the cylinder by any given pull. Thus as the number of turns increases, the line of contact becomes longer and in consequence more resistant; so that the cord slips and yields to the tractive force with increasing difficulty.

Is it not clear that this is precisely the kind of resistance which one meets in the case of a thick hemp rope where the fibres form thousands and thousands of similar spirals? And, indeed, the binding effect of these turns is so great that a few short rushes woven together into a few interlacing spirals form one of the strongest of ropes which I believe they call pack rope.

SAGREDO. What you say has cleared up two points which I did not previously understand. One fact is how two, or at most three, turns of a rope around the axle of a windlass cannot only hold it fast, but can also prevent it from slipping when pulled by the immense force of the weight which it sustains; and moreover how, by turning the windlass, this same axle, by mere friction of the rope around it, can wind up and lift huge stones while a mere boy is able to handle the slack of the rope. The other fact has to do with a simple but clever device, invented by a young kinsman of mine, for the purpose of descending from a window by means of a rope without lacerating the palms of his hands, as had happened to him shortly before and greatly to his discomfort. A small sketch will make this clear. He took a wooden cylinder, AB, about as thick as a walking stick and about one span long: on this he cut a spiral channel of about one turn and a half, and large enough to just receive the rope which he wished to use. Having introduced the rope at the end A and led it out again at the end B, he enclosed both the cylinder and the rope in a case of wood or tin, hinged along the side so that it could be easily opened and closed. After he had fastened the rope to a firm support above, he could, on grasping and squeezing the case with both hands, hang by his arms. The pressure on the rope, lying between the case and the cylinder, was such that he could, at will, either grasp the case more tightly and hold himself from slipping, or slacken his hold and descend as slowly as he wished.



SALVIATI. A truly ingenious device! I feel, however, that for a complete explanation other considerations might well enter; yet I must not now digress upon this particular topic since you are waiting to hear what I think about the breaking strength of other materials which, unlike ropes and most woods, do not show a filamentous structure. The coherence of these bodies is, in my estimation, produced by other causes which may be grouped under two heads. One is that much-talked-of repugnance which nature exhibits towards a vacuum; but this horror of a vacuum not being sufficient, it is necessary to introduce another cause in the form of a gluey or viscous substance which binds firmly together the component parts of the body.

First I shall speak of the vacuum, demonstrating by definite experiment the quality and quantity of its force. If you take two highly polished and smooth plates of marble, metal, or glass and place them face to face, one will slide over the other with the greatest ease, showing conclusively that there is nothing of a viscous nature between them. But when you attempt to separate them and keep them at a constant distance apart, you find the plates exhibit such a repugnance to separation that the upper one will carry the lower one with it and keep it lifted indefinitely, even when the latter is big and heavy.

This experiment shows the aversion of nature for empty space, even during the brief moment required for the outside air to rush in and fill up the region between the two plates. It is also observed that if two plates are not thoroughly polished, their contact is imperfect so that when you attempt to separate them slowly the only resistance offered is that of weight; if, however, the pull be sudden, then the lower plate rises, but quickly falls back, having followed the upper plate only for that very short interval of time required for the expansion of the small amount of air remaining between the plates, in consequence of their not fitting, and for the entrance of the surrounding air. This resistance which is exhibited between the two plates is doubtless likewise present between the parts of a solid, and enters, at least in part, as a concomitant cause of their coherence.

SAGREDO. Allow me to interrupt you for a moment, please; for I want to speak of something which just occurs to me, namely, when I see how the lower plate follows the upper one and how rapidly it is lifted, I feel sure that, contrary to the opinion of many philosophers, including perhaps even Aristotle himself, motion in a vacuum is not instantaneous. If this were so the two plates mentioned above would separate without any resistance whatever, seeing that the same instant of time would suffice for their separation and for the surrounding medium to rush in and fill the vacuum between them. The fact that the lower plate follows the upper one allows us to infer, not only that motion in a vacuum is not instantaneous, but also that, between the two plates, a vacuum really exists, at least for a very short time, sufficient to allow the surrounding medium to rush in and fill the vacuum; for if there were no vacuum there would be no need of any motion in the medium. One must admit then that a vacuum

is sometimes produced by violent motion or contrary to the laws of nature (although in my opinion nothing occurs contrary to nature except the impossible, and that never occurs).

But here another difficulty arises. While experiment convinces me of the correctness of this conclusion, my mind is not entirely satisfied as to the cause to which this effect is to be attributed. For the separation of the plates precedes the formation of the vacuum which is produced as a consequence of this separation; and since it appears to me that, in the order of nature, the cause must precede the effect, even though it appears to follow in point of time, and since every positive effect must have a positive cause, I do not see how the adhesion of two plates and their resistance to separation—actual facts—can be referred to a vacuum as cause when this vacuum is yet to follow. According to the infallible maxim of the Philosopher, the non-existent can produce no effect.

SIMPLICIO. Seeing that you accept this axiom of Aristotle, I hardly think you will reject another excellent and reliable maxim of his, namely, Nature undertakes only that which happens without resistance; and in this saying, it appears to me, you will find the solution of your difficulty. Since nature abhors a vacuum, she prevents that from which a vacuum would follow as a necessary consequence. Thus it happens that nature prevents the separation of the two plates.

SAGREDO. Now admitting that what Simplicio says is an adequate solution of my difficulty, it seems to me, if I may be allowed to resume my former argument, that this very resistance to a vacuum ought to be sufficient to hold together the parts either of stone or of metal or the parts of any other solid which is knit together more strongly and which is more resistant to separation. If for one effect there be only one cause, or if, more being assigned, they can be reduced to one, then why is not this vacuum which really exists a sufficient cause for all kinds of resistance?

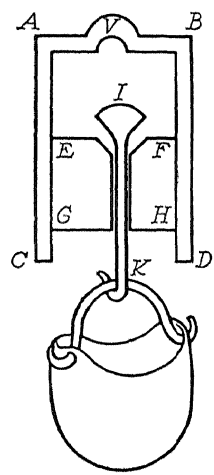
SALVIATI. I do not wish just now to enter this discussion as to whether the vacuum alone is sufficient to hold together the separate parts of a solid body; but I assure you that the vacuum which acts as a sufficient cause in the case of the two plates is not alone sufficient to bind together the parts of a solid cylinder of marble or metal which, when pulled violently, separates and divides. And now if I find a method of distinguishing this well known resistance, depending upon the vacuum, from every other kind which might increase the coherence, and if I show you that the aforesaid resistance alone is not nearly sufficient for such an effect, will you not grant that we are bound to introduce another cause? Help him, Simplicio, since he does not know what reply to make.

SIMPLICIO. Surely, Sagredo's hesitation must be owing to another reason, for there can be no doubt concerning a conclusion which is at once so clear and logical.

SAGREDO. You have guessed rightly, Simplicio. I was wondering whether, if a million of gold each year from Spain were not sufficient to pay the army, it might not be necessary to make provision other than small coin for the pay of the soldiers.

But go ahead, Salviati; assume that I admit your conclusion and show us your method of separating the action of the vacuum from other causes; and by measuring it show us how it is not sufficient to produce the effect in question.

SALVIATI. Your good angel assist you. I will tell you how to separate the force of the vacuum from the others, and afterwards how to measure it. For this purpose let us consider a continuous substance whose parts lack all resistance to separation except that derived from a vacuum, such as is the case with water, a fact fully demonstrated by our Academician in one of his treatises. Whenever a cylinder of water is subjected to a pull and offers a resistance to the separation of its parts this can be attributed to no other cause than the resistance of the vacuum. In order to try such an experiment I have invented a device which I can better explain by means of a sketch than by mere words. Let CABD represent the cross section of a cylinder either of metal or, preferably, of glass, hollow inside and accurately turned. Into this is introduced a perfectly fitting cylinder of wood, represented in cross section by EGHF, and capable of up-and-down motion. Through the middle of this cylinder is bored a hole to receive an iron wire, carrying a hook at the end K, while the upper end of the wire, I, is provided with a conical head. The wooden cylinder is countersunk at the top so as to receive, with a perfect fit, the conical head I of the wire, IK, when pulled down by the end K.



Now insert the wooden cylinder EH in the hollow cylinder AD, so as not to touch the upper end of the latter but to leave free a space of two or three fingerbreadths; this space is to be filled with water by holding the vessel with the mouth CD upwards, pushing down on the stopper EH, and at the same time keeping the conical head of the wire, I, away from the hollow portion of the wooden cylinder. The air is thus allowed to escape alongside the iron wire (which does not make a close fit) as soon as one presses down on the wooden stopper. The air having been allowed to escape and the iron wire having been drawn back so that it fits snugly against the conical depression in the wood, invert the vessel, bringing it mouth downwards, and hang on the hook K a vessel which can be filled with sand or any heavy material in quantity sufficient to finally separate the upper surface of the stopper, EF, from the lower surface of the water to which it was attached only by the resistance of the vacuum. Next weigh the stopper and wire together with the attached vessel and its contents; we shall then have the force of the vacuum. If one attaches to a cylinder of marble or glass a weight which, together with the weight of the marble or glass itself, is just equal to the sum of the weights before mentioned, and if breaking occurs we shall then be justi-

fied in saying that the vacuum alone holds the parts of the marble and glass together; but if this weight does not suffice and if breaking occurs only after adding, say, four times this weight, we shall then be compelled to say that the vacuum furnishes only one fifth of the total resistance.

SIMPLICIO. No one can doubt the cleverness of the device; yet it presents many difficulties which make me doubt its reliability. For who will assure us that the air does not creep in between the glass and stopper even if it is well packed with tow or other yielding material? I question also whether oiling with wax or turpentine will suffice to make the cone, I, fit snugly on its seat. Besides, may not the parts of the water expand and dilate? Why may not the air or exhalations or some other more subtile substances penetrate the pores of the wood, or even of the glass itself?

SALVIATI. With great skill indeed has Simplicio laid before us the difficulties; and he has even partly suggested how to prevent the air from penetrating the wood or passing between the wood and the glass. But now let me point out that, as our experience increases, we shall learn whether or not these alleged difficulties really exist. For if, as is the case with air, water is by nature expansible, although only under severe treatment, we shall see the stopper descend; and if we put a small excavation in the upper part of the glass vessel, such as indicated by V, then the air or any other tenuous and gaseous substance, which might penetrate the pores of glass or wood, would pass through the water and collect in this receptacle V. But if these things do not happen we may rest assured that our experiment has been performed with proper caution; and we shall discover that water does not dilate and that glass does not allow any material, however tenuous, to penetrate it.

SAGREDO. Thanks to this discussion, I have learned the cause of a certain effect which I have long wondered at and despaired of understanding. I once saw a cistern which had been provided with a pump under the mistaken impression that the water might thus be drawn with less effort or in greater quantity than by means of the ordinary bucket. The stock of the pump carried its sucker and valve in the upper part so that the water was lifted by attraction and not by a push as is the case with pumps in which the sucker is placed lower down. This pump worked perfectly so long as the water in the cistern stood above a certain level; but below this level the pump failed to work. When I first noticed this phenomenon I thought the machine was out of order; but the workman whom I called in to repair it told me the defect was not in the pump but in the water which had fallen too low to be raised through such a height; and he added that it was not possible, either by a pump or by any other machine working on the principle of attraction, to lift water a hair's breadth above eighteen cubits; whether the pump be large or small this is the extreme limit of the lift. Up to this time I had been so thoughtless that, although I knew a rope, or rod of wood, or of iron, if sufficiently long, would break by its own weight when held by the upper end, it never occurred to me that the same thing would happen, only much more

easily, to a column of water. And really is not that thing which is attracted in the pump a column of water attached at the upper end and stretched more and more until finally a point is reached where it breaks, like a rope, on account of its excessive weight?

SALVIATI. That is precisely the way it works; this fixed elevation of eighteen cubits is true for any quantity of water whatever, be the pump large or small or even as fine as a straw. We may therefore say that, on weighing the water contained in a tube eighteen cubits long, no matter what the diameter, we shall obtain the value of the resistance of the vacuum in a cylinder of any solid material having a bore of this same diameter. And having gone so far, let us see how easy it is to find to what length cylinders of metal, stone, wood, glass, etc., of any diameter can be elongated without breaking by their own weight.

Take for instance a copper wire of any length and thickness; fix the upper end and to the other end attach a greater and greater load until finally the wire breaks; let the maximum load be, say, fifty pounds. Then it is clear that if fifty pounds of copper, in addition to the weight of the wire itself which may be, say, ⅛ ounce, is drawn out into wire of this same size we shall have the greatest length of this kind of wire which can sustain its own weight. Suppose the wire which breaks to be one cubit in length and ⅛ ounce in weight; then since it supports 50 lbs. in addition to its own weight, i. e., 4800 eighths-of-an-ounce, it follows that all copper wires, independent of size, can sustain themselves up to a length of 4801 cubits and no more. Since then a copper rod can sustain its own weight up to a length of 4801 cubits it follows that that part of the breaking strength which depends upon the vacuum, comparing it with the remaining factors of resistance, is equal to the weight of a rod of water, eighteen cubits long and as thick as the copper rod. If, for example, copper is nine times as heavy as water, the breaking strength of any copper rod, in so far as it depends upon the vacuum, is equal to the weight of two cubits of this same rod. By a similar method one can find the maximum length of wire or rod of any material which will just sustain its own weight, and can at the same time discover the part which the vacuum plays in its breaking strength.

SAGREDO. It still remains for you to tell us upon what depends the resistance to breaking, other than that of the vacuum; what is the gluey or viscous substance which cements together the parts of the solid? For I cannot imagine a glue that will not burn up in a highly heated furnace in two or three months, or certainly within ten or a hundred. For if gold, silver and glass are kept for a long while in the molten state and are removed from the furnace, their parts, on cooling, immediately reunite and bind themselves together as before. Not only so, but whatever difficulty arises with respect to the cementation of the parts of the glass arises also with regard to the parts of the glue; in other words, what is that which holds these parts together so firmly?

SALVIATI. A little while ago, I expressed the hope that your good

angel might assist you. I now find myself in the same straits. Experiment leaves no doubt that the reason why two plates cannot be separated, except with violent effort, is that they are held together by the resistance of the vacuum; and the same can be said of two large pieces of a marble or bronze column. This being so, I do not see why this same cause may not explain the coherence of smaller parts and indeed of the very smallest particles of these materials. Now, since each effect must have one true and sufficient cause and since I find no other cement, am I not justified in trying to discover whether the vacuum is not a sufficient cause?

SIMPLICIO. But seeing that you have already proved that the resistance which the large vacuum offers to the separation of two large parts of a solid is really very small in comparison with that cohesive force which binds together the most minute parts, why do you hesitate to regard this latter as something very different from the former?

SALVIATI. Sagredo has already answered this question when he remarked that each individual soldier was being paid from coin collected by a general tax of pennies and farthings, while even a million of gold would not suffice to pay the entire army. And who knows but that there may be other extremely minute vacua which affect the smallest particles so that that which binds together the contiguous parts is throughout of the same mintage?

In reply to the question raised by Simplicio, one may say that although each particular vacuum is exceedingly minute and therefore easily overcome, yet their number is so extraordinarily great that their combined resistance is, so to speak, multipled almost without limit. The nature and the amount of force which results from adding together an immense number of small forces is clearly illustrated by the fact that a weight of millions of pounds, suspended by great cables, is overcome and lifted, when the south wind carries innumerable atoms of water, suspended in thin mist, which moving through the air penetrate between the fibres of the tense ropes in spite of the tremendous force of the hanging weight. When these particles enter the narrow pores they swell the ropes, thereby shorten them, and perforce lift the heavy mass.

SAGREDO. There can be no doubt that any resistance, so long as it is not infinite, may be overcome by a multitude of minute forces. Thus a vast number of ants might carry ashore a ship laden with grain. And since experience shows us daily that one ant can easily carry one grain, it is clear that the number of grains in the ship is not infinite, but falls below a certain limit. If you take another number four or six times as great, and if you set to work a corresponding number of ants they will carry the grain ashore and the boat also. It is true that this will call for a prodigious number of ants, but in my opinion this is precisely the case with the vacua which bind together the least particles of a metal.

SALVIATI. But even if this demanded an infinite number would you still think it impossible?

SAGREDO. Not if the mass of metal were infinite.

SAGREDO. The phenomenon of light is one which I have many times remarked with astonishment. I have, for instance, seen lead melted instantly by means of a concave mirror only three hands in diameter. Hence I think that if the mirror were very large, well polished and of a parabolic figure, it would just as readily and quickly melt any other metal, seeing that the small mirror, which was not well polished and had only a spherical shape, was able so energetically to melt lead and burn every combustible substance. Such effects as these render credible to me the marvels accomplished by the mirrors of Archimedes.

SALVIATI. Speaking of the effects produced by the mirrors of Archimedes, it was his own books (which I had already read and studied with infinite astonishment) that rendered credible to me all the miracles described by various writers. And if any doubt had remained the book which Father Buonaventura Cavalieri has recently published on the subject of the burning glass and which I have read with admiration would have removed the last difficulty.

SAGREDO. I also have seen this treatise and have read it with pleasure and astonishment; and knowing the author I was confirmed in the opinion which I had already formed of him that he was destined to become one of the leading mathematicians of our age. But now, with regard to the surprising effect of solar rays in melting metals, must we believe that such a furious action is devoid of motion or that it is accompanied by the most rapid of motions?

SALVIATI. We observe that other combustions and resolutions are accompanied by motion, and that, the most rapid; note the action of lightning and of powder as used in mines and petards; note also how the charcoal flame, mixed as it is with heavy and impure vapors, increases its power to liquefy metals whenever quickened by a pair of bellows. Hence I do not understand how the action of light, although very pure, can be devoid of motion and that of the swiftest type.

SAGREDO. But of what kind and how great must we consider this speed of light to be? Is it instantaneous or momentary or does it like other motions require time? Can we not decide this by experiment?

SIMPLICIO. Everyday experience shows that the propagation of light is instantaneous; for when we see a piece of artillery fired, at great distance, the flash reaches our eyes without lapse of time; but the sound reaches the ear only after a noticeable interval.

SAGREDO. Well, Simplicio, the only thing I am able to infer from this familiar bit of experience is that sound, in reaching our ear, travels more slowly than light; it does not inform me whether the coming of the light is instantaneous or whether, although extremely rapid, it still occupies time. An observation of this kind tells us nothing more than one in which it is claimed that "As soon as the sun reaches the horizon its light reaches our eyes"; but who will assure me that these rays had not reached this limit earlier than they reached our vision?

SALVIATI. The small conclusiveness of these and other similar observations once led me to devise a method by which one might accurately

ascertain whether illumination, i. e., the propagation of light, is really instantaneous. The fact that the speed of sound is as high as it is, assures us that the motion of light cannot fail to be extraordinarily swift. The experiment which I devised was as follows:

Let each of two persons take a light contained in a lantern, or other receptacle, such that by the interposition of the hand, the one can shut off or admit the light to the vision of the other. Next let them stand opposite each other at a distance of a few cubits and practice until they acquire such skill in uncovering and occulting their lights that the instant one sees the light of his companion he will uncover his own. After a few trials the response will be so prompt that without sensible error the uncovering of one light is immediately followed by the uncovering of the other, so that as soon as one exposes his light he will instantly see that of the other. Having acquired skill at this short distance let the two experimenters, equipped as before, take up positions separated by a distance of two or three miles and let them perform the same experiment at night, noting carefully whether the exposures and occultations occur in the same manner as at short distances; if they do, we may safely conclude that the propagation of light is instantaneous; but if time is required at a distance of three miles which, considering the going of one light and the coming of the other, really amounts to six, then the delay ought to be easily observable. If the experiment is to be made at still greater distances, say eight or ten miles, telescopes may be employed, each observer adjusting one for himself at the place where he is to make the experiment at night; then although the lights are not large and are therefore invisible to the naked eye at so great a distance, they can readily be covered and uncovered since by aid of the telescopes, once adjusted and fixed, they will become easily visible.

SAGREDO. This experiment strikes me as a clever and reliable invention. But tell us what you conclude from the results.

SALVIATI. In fact I have tried the experiment only at a short distance, less than a mile, from which I have not been able to ascertain with certainty whether the appearance of the opposite light was instantaneous or not; but if not instantaneous it is extraordinarily rapid—I should call it momentary; and for the present I should compare it to motion which we see in the lightning flash between clouds eight or ten miles distant from us. We see the beginning of this light—I might say its head and source—located at a particular place among the clouds; but it immediately spreads to the surrounding ones, which seems to be an argument that at least some time is required for propagation; for if the illumination were instantaneous and not gradual, we should not be able to distinguish its origin—its center, so to speak—from its outlying portions.

SAGREDO. I quite agree with the peripatetic philosophers in denying the penetrability of matter. As to the vacua I should like to hear a thorough discussion of Aristotle's demonstration in which he opposes them, and what you, Salviati, have to say in reply. I beg of you, Simplicio,

that you give us the precise proof of the Philosopher and that you, Salviati, give us the reply.

SIMPLICIO. So far as I remember, Aristotle inveighs against the ancient view that a vacuum is a necessary prerequisite for motion and that the latter could not occur without the former. In opposition to this view Aristotle shows that it is precisely the phenomenon of motion, as we shall see, which renders untenable the idea of a vacuum. His method is to divide the argument into two parts. He first supposes bodies of different weight to move in the same medium; then supposes, one and the same body to move in different media. In the first case, he supposes bodies of different weight to move in one and the same medium with different speeds which stand to one another in the same ratio as the weights; so that, for example, a body which is ten times as heavy as another will move ten times as rapidly as the other. In the second case he assumes that the speeds of one and the same body moving in different media are in inverse ratio to the densities of these media; thus, for instance, if the density of water were ten times that of air, the speed in air would be ten times greater than in water. From this second supposition, he shows that, since the tenuity of a vacuum differs infinitely from that of any medium filled with matter however rare, any body which moves in a plenum through a certain space in a certain time ought to move through a vacuum instantaneously; but instantaneous motion is an impossibility; it is therefore impossible that a vacuum should be produced by motion.

SALVIATI. The argument is, as you see, *ad hominem,* that is, it is directed against those who thought the vacuum a prerequisite for motion. Now if I admit the argument to be conclusive and concede also that motion cannot take place in a vacuum, the assumption of a vacuum considered absolutely and not with reference to motion, is not thereby invalidated. But to tell you what the ancients might possibly have replied and in order to better understand just how conclusive Aristotle's demonstration is, we may, in my opinion, deny both of his assumptions. And as to the first, I greatly doubt that Aristotle ever tested by experiment whether it be true that two stones, one weighing ten times as much as the other, if allowed to fall, at the same instant, from a height of, say, 100 cubits, would so differ in speed that when the heavier had reached the ground, the other would not have fallen more than 10 cubits.

SIMPLICIO. His language would seem to indicate that he had tried the experiment, because he says: *We see the heavier;* now the word *see* shows that he had made the experiment.

SAGREDO. But I, Simplicio, who have made the test can assure you that a cannon ball weighing one or two hundred pounds, or even more, will not reach the ground by as much as a span ahead of a musket ball weighing only half a pound, provided both are dropped from a height of 200 cubits.

SALVIATI. But, even without further experiment, it is possible to prove

clearly, by means of a short and conclusive argument, that a heavier body does not move more rapidly than a lighter one provided both bodies are of the same material and in short such as those mentioned by Aristotle. But tell me, Simplicio, whether you admit that each falling body acquires a definite speed fixed by nature, a velocity which cannot be increased or diminished except by the use of force or resistance.

SIMPLICIO. There can be no doubt but that one and the same body moving in a single medium has a fixed velocity which is determined by nature and which cannot be increased except by the addition of momentum or diminished except by some resistance which retards it.

SALVIATI. If then we take two bodies whose natural speeds are different, it is clear that on uniting the two, the more rapid one will be partly retarded by the slower, and the slower will be somewhat hastened by the swifter. Do you not agree with me in this opinion?

SIMPLICIO. You are unquestionably right.

SALVIATI. But if this is true, and if a large stone moves with a speed of, say, eight while a smaller moves with a speed of four, then when they are united, the system will move with a speed less than eight; but the two stones when tied together make a stone larger than that which before moved with a speed of eight. Hence the heavier body moves with less speed than the lighter; an effect which is contrary to your supposition. Thus you see how, from your assumption that the heavier body moves more rapidly than the lighter one. I infer that the heavier body moves more slowly.

SIMPLICIO. I am all at sea because it appears to me that the smaller stone when added to the larger increases its weight and by adding weight I do not see how it can fail to increase its speed or, at least, not to diminish it.

SALVIATI. Here again you are in error, Simplicio, because it is not true that the smaller stone adds weight to the larger.

SIMPLICIO. This is, indeed, quite beyond my comprehension.

SALVIATI. It will not be beyond you when I have once shown you the mistake under which you are laboring. Note that it is necessary to distinguish between heavy bodies in motion and the same bodies at rest. A large stone placed in a balance not only acquires additional weight by having another stone placed upon it, but even by the addition of a handful of hemp its weight is augmented six to ten ounces according to the quantity of hemp. But if you tie the hemp to the stone and allow them to fall freely from some height, do you believe that the hemp will press down upon the stone and thus accelerate its motion or do you think the motion will be retarded by a partial upward pressure? One always feels the pressure upon his shoulders when he prevents the motion of a load resting upon him; but if one descends just as rapidly as the load would fall how can it gravitate or press upon him? Do you not see that this would be the same as trying to strike a man with a lance when he is running away from you with a speed which is equal to, or even greater, than that with which you are following him? You must therefore conclude that,

during free and natural fall, the small stone does not press upon the larger and consequently does not increase its weight as it does when at rest.

SIMPLICIO. But what if we should place the larger stone upon the smaller?

SALVIATI. Its weight would be increased if the larger stone moved more rapidly; but we have already concluded that when the small stone moves more slowly it retards to some extent the speed of the larger, so that the combination of the two, which is a heavier body than the larger of the two stones, would move less rapidly, a conclusion which is contrary to your hypothesis. We infer therefore that large and small bodies move with the same speed provided they are of the same specific gravity.

SIMPLICIO. Your discussion is really admirable; yet I do not find it easy to believe that a bird shot falls as swiftly as a cannon ball.

SALVIATI. Why not say a grain of sand as rapidly as a grindstone? But, Simplicio, I trust you will not follow the example of many others who divert the discussion from its main intent and fasten upon some statement of mine which lacks a hairsbreadth of the truth and, under this hair, hide the fault of another which is as big as a ship's cable. Aristotle says that "an iron ball of one hundred pounds falling from a height of one hundred cubits reaches the ground before a one-pound ball has fallen a single cubit." I say that they arrive at the same time. You find, on making the experiment, that the larger outstrips the smaller by two finger-breadths, that is, when the larger has reached the ground, the other is short of it by two fingerbreadths; now you would not hide behind these two fingers the ninety-nine cubits of Aristotle, nor would you mention my small error and at the same time pass over in silence his very large one. Aristotle declares that bodies of different weights, in the same medium, travel (in so far as their motion depends upon gravity) with speeds which are proportional to their weights; this he illustrates by use of bodies in which it is possible to perceive the pure and unadulterated effect of gravity, eliminating other considerations, for example, figure, as being of small importance, influences which are greatly dependent upon the medium which modifies the single effect of gravity alone. Thus we observe that gold, the densest of all substances, when beaten out into a very thin leaf, goes floating through the air; the same thing happens with stone when ground into a very fine powder. But if you wish to maintain the general proposition you will have to show that the same ratio of speeds is preserved in the case of all heavy bodies, and that a stone of twenty pounds moves ten times as rapidly as one of two; but I claim that this is false and that, if they fall from a height of fifty or a hundred cubits, they will reach the earth at the same moment.

SIMPLICIO. Perhaps the result would be different if the fall took place not from a few cubits but from some thousands of cubits.

SALVIATI. If this were what Aristotle meant you would burden him with another error which would amount to a falsehood; because, since there is no such sheer height available on earth, it is clear that Aristotle could not have made the experiment; yet he wishes to give us the impres-

sion of his having performed it when he speaks of such an effect as one which we see.

SIMPLICIO. In fact, Aristotle does not employ this principle, but uses the other one which is not, I believe, subject to these same difficulties.

SALVIATI. But the one is as false as the other; and I am surprised that you yourself do not see the fallacy and that you do not perceive that if it were true that, in media of different densities and different resistances, such as water and air, one and the same body moved in air more rapidly than in water, in proportion as the density of water is greater than that of air, then it would follow that any body which falls through air ought also to fall through water. But this conclusion is false inasmuch as many bodies which descend in air not only do not descend in water, but actually rise.

SIMPLICIO. I do not understand the necessity of your inference; and in addition I will say that Aristotle discusses only those bodies which fall in both media, not those which fall in air but rise in water.

SALVIATI. The arguments which you advance for the Philosopher are such as he himself would have certainly avoided so as not to aggravate his first mistake. But tell me now whether the density of the water, or whatever it may be that retards the motion, bears a definite ratio to the density of air which is less retardative; and if so fix a value for it at your pleasure.

SIMPLICIO. Such a ratio does exist; let us assume it to be ten; then, for a body which falls in both these media, the speed in water will be ten times slower than in air.

SALVIATI. I shall now take one of those bodies which fall in air but not in water, say a wooden ball, and I shall ask you to assign to it any speed you please for its descent through air.

SIMPLICIO. Let us suppose it moves with a speed of twenty.

SALVIATI. Very well. Then it is clear that this speed bears to some smaller speed the same ratio as the density of water bears to that of air; and the value of this smaller speed is two. So that really if we follow exactly the assumption of Aristotle we ought to infer that the wooden ball which falls in air, a substance ten times less-resisting than water, with a speed of twenty would fall in water with a speed of two, instead of coming to the surface from the bottom as it does; unless perhaps you wish to reply, which I do not believe you will, that the rising of the wood through the water is the same as its falling with a speed of two. But since the wooden ball does not go to the bottom, I think you will agree with me that we can find a ball of another material, not wood, which does fall in water with a speed of two.

SIMPLICIO. Undoubtedly we can; but it must be of a substance considerably heavier than wood.

SALVIATI. That is it exactly. But if this second ball falls in water with a speed of two, what will be its speed of descent in air? If you hold to the rule of Aristotle you must reply that it will move at the rate of twenty; but twenty is the speed which you yourself have already assigned

to the wooden ball; hence this and the other heavier ball will each move through air with the same speed. But now how does the Philosopher harmonize this result with his other, namely, that bodies of different weight move through the same medium with different speeds—speeds which are proportional to their weights? But without going into the matter more deeply, how have these common and obvious properties escaped your notice? Have you not observed that two bodies which fall in water, one with a speed a hundred times as great as that of the other, will fall in air with speeds so nearly equal that one will not surpass the other by as much as one hundredth part? Thus, for example, an egg made of marble will descend in water one hundred times more rapidly than a hen's egg, while in air falling from a height of twenty cubits the one will fall short of the other by less than four fingerbreadths. In short, a heavy body which sinks through ten cubits of water in three hours will traverse ten cubits of air in one or two pulse beats; and if the heavy body be a ball of lead it will easily traverse the ten cubits of water in less than double the time required for ten cubits of air. And here, I am sure, Simplicio, you find no ground for difference or objection. We conclude, therefore, that the argument does not bear against the existence of a vacuum; but if it did, it would only do away with vacua of considerable size which neither I nor, in my opinion, the ancients ever believed to exist in nature, although they might possibly be produced by force as may be gathered from various experiments whose description would here occupy too much time.

SAGREDO. Seeing that Simplicio is silent, I will take the opportunity of saying something. Since you have clearly demonstrated that bodies of different weights do not move in one and the same medium with velocities proportional to their weights, but that they all move with the same speed, understanding of course that they are of the same substance or at least of the same specific gravity; certainly not of different specific gravities, for I hardly think you would have us believe a ball of cork moves with the same speed as one of lead; and again since you have clearly demonstrated that one and the same body moving through differently resisting media does not acquire speeds which are inversely proportional to the resistances, I am curious to learn what are the ratios actually observed in these cases.

SALVIATI. These are interesting questions and I have thought much concerning them. I will give you the method of approach and the result which I finally reached. Having once established the falsity of the proposition that one and the same body moving through differently resisting media acquires speeds which are inversely proportional to the resistances of these media, and having also disproved the statement that in the same medium bodies of different weight acquire velocities proportional to their weights (understanding that this applies also to bodies which differ merely in specific gravity), I then began to combine these two facts and to consider what would happen if bodies of different weight were placed in media of different resistances; and I found that the differences in speed

were greater in those media which were more resistant, that is, less yielding. This difference was such that two bodies which differed scarcely at all in their speed through air would, in water, fall the one with a speed ten times as great as that of the other. Further, there are bodies which will fall rapidly in air, whereas if placed in water not only will not sink but will remain at rest or will even rise to the top: for it is possible to find some kinds of wood, such as knots and roots, which remain at rest in water but fall rapidly in air.

SAGREDO. I have often tried with the utmost patience to add grains of sand to a ball of wax until it should acquire the same specific gravity as water and would therefore remain at rest in this medium. But with all my care I was never able to accomplish this. Indeed, I do not know whether there is any solid substance whose specific gravity is, by nature, so nearly equal to that of water that if placed anywhere in water it will remain at rest.

SALVIATI. In this, as in a thousand other operations, men are surpassed by animals. In this problem of yours one may learn much from the fish which are very skillful in maintaining their equilibrium not only in one kind of water, but also in waters which are notably different either by their own nature or by some accidental muddiness or through salinity, each of which produces a marked change. So perfectly indeed can fish keep their equilibrium that they are able to remain motionless in any position. This they accomplish, I believe, by means of an apparatus especially provided by nature, namely, a bladder located in the body and communicating with the mouth by means of a narrow tube through which they are able, at will, to expel a portion of the air contained in the bladder: by rising to the surface they can take in more air; thus they make themselves heavier or lighter than water at will and maintain equilibrium.

SAGREDO. By means of another device I was able to deceive some friends to whom I had boasted that I could make up a ball of wax that would be in equilibrium in water. In the bottom of a vessel I placed some salt water and upon this some fresh water; then I showed them that the ball stopped in the middle of the water, and that, when pushed to the bottom or lifted to the top, it would not remain in either of these places but would return to the middle.

SALVIATI. This experiment is not without usefulness. For when physicians are testing the various qualities of waters, especially their specific gravities, they employ a ball of this kind so adjusted that, in certain water, it will neither rise nor fall. Then in testing another water, differing ever so slightly in specific gravity, the ball will sink if this water be lighter and rise if it be heavier. And so exact is this experiment that the addition of two grains of salt to six pounds of water is sufficient to make the ball rise to the surface from the bottom to which it had fallen. To illustrate the precision of this experiment and also to clearly demonstrate the nonresistance of water to division, I wish to add that this notable difference in specific gravity can be produced not only by solution of some heavier substance, but also by merely heating or cooling; and so sensitive is water

to this process that by simply adding four drops of another water which is slightly warmer or cooler than the six pounds one can cause the ball to sink or rise; it will sink when the warm water is poured in and will rise upon the addition of cold water. Now you can see how mistaken are those philosophers who ascribe to water viscosity or some other coherence of parts which offers resistance to separation of parts and to penetration.

SAGREDO. With regard to this question I have found many convincing arguments in a treatise by our Academician; but there is one great difficulty of which I have not been able to rid myself, namely, if there be no tenacity or coherence between the particles of water how is it possible for those large drops of water to stand out in relief upon cabbage leaves without scattering or spreading out?

SALVIATI. Although those who are in possession of the truth are able to solve all objections raised, I would not arrogate to myself such power; nevertheless my inability should not be allowed to becloud the truth. To begin with let me confess that I do not understand how these large globules of water stand out and hold themselves up, although I know for a certainty that it is not owing to any internal tenacity acting between the particles of water; whence it must follow that the cause of this effect is external. Beside the experiments already shown to prove that the cause is not internal, I can offer another which is very convincing. If the particles of water which sustain themselves in a heap, while surrounded by air, did so in virtue of an internal cause then they would sustain themselves much more easily when surrounded by a medium in which they exhibit less tendency to fall than they do in air; such a medium would be any fluid heavier than air, as, for instance, wine: and therefore if some wine be poured about such a drop of water, the wine might rise until the drop was entirely covered, without the particles of water, held together by this internal coherence, ever parting company. But this is not the fact; for as soon as the wine touches the water, the latter without waiting to be covered scatters and spreads out underneath the wine if it be red. The cause of this effect is therefore external and is possibly to be found in the surrounding air. Indeed there appears to be a considerable antagonism between air and water as I have observed in the following experiment. Having taken a glass globe which had a mouth of about the same diameter as a straw, I filled it with water and turned it mouth downwards; nevertheless, the water, although quite heavy and prone to descend, and the air, which is very light and disposed to rise through the water, refused, the one to descend and the other to ascend through the opening, but both remained stubborn and defiant. On the other hand, as soon as I apply to this opening a glass of red wine, which is almost inappreciably lighter than water, red streaks are immediately observed to ascend slowly through the water while the water with equal slowness descends through the wine without mixing, until finally the globe is completely filled with wine and the water has all gone down into the vessel below. What then can we say except that there exists, between water and air, a certain incompatibility which I do not understand, but perhaps . . .

SIMPLICIO. I feel almost like laughing at the great antipathy which Salviati exhibits against the use of the word antipathy; and yet it is excellently adapted to explain the difficulty.

SALVIATI. All right, if it please Simplicio, let this word antipathy be the solution of our difficulty. Returning from this digression, let us again take up our problem. We have already seen that the difference of speed between bodies of different specific gravities is most marked in those media which are the most resistant: thus, in a medium of quicksilver, gold not merely sinks to the bottom more rapidly than lead but it is the only substance that will descend at all; all other metals and stones rise to the surface and float. On the other hand the variation of speed in air between balls of gold, lead, copper, porphyry, and other heavy materials is so slight that in a fall of 100 cubits a ball of gold would surely not outstrip one of copper by as much as four fingers. Having observed this I came to the conclusion that in a medium totally devoid of resistance all bodies would fall with the same speed.

SIMPLICIO. This is a remarkable statement, Salviati. But I shall never believe that even in a vacuum, if motion in such a place were possible, a lock of wool and a bit of lead can fall with the same velocity.

SALVIATI. A little more slowly, Simplicio. Your difficulty is not so recondite nor am I so imprudent as to warrant you in believing that I have not already considered this matter and found the proper solution. Hence for my justification and for your enlightenment hear what I have to say. Our problem is to find out what happens to bodies of different weight moving in a medium devoid of resistance, so that the only difference in speed is that which arises from inequality of weight. Since no medium except one entirely free from air and other bodies, be it ever so tenuous and yielding, can furnish our senses with the evidence we are looking for, and since such a medium is not available, we shall observe what happens in the rarest and least resistant media as compared with what happens in denser and more resistant media. Because if we find as a fact that the variation of speed among bodies of different specific gravities is less and less according as the medium becomes more and more yielding, and if finally in a medium of extreme tenuity, though not a perfect vacuum, we find that, in spite of great diversity of specific gravity, the difference in speed is very small and almost inappreciable, then we are justified in believing it highly probable that in a vacuum all bodies would fall with the same speed. Let us, in view of this, consider what takes place in air, where for the sake of a definite figure and light material imagine an inflated bladder. The air in this bladder when surrounded by air will weigh little or nothing, since it can be only slightly compressed; its weight then is small being merely that of the skin which does not amount to the thousandth part of a mass of lead having the same size as the inflated bladder. Now, Simplicio, if we allow these two bodies to fall from a height of four or six cubits, by what distance do you imagine the lead will anticipate the bladder? You may be sure that the lead will

not travel three times, or even twice, as swiftly as the bladder, although you would have made it move a thousand times as rapidly.

SIMPLICIO. It may be as you say during the first four or six cubits of the fall; but after the motion has continued a long while, I believe that the lead will have left the bladder behind not only six out of twelve parts of the distance but even eight or ten.

SALVIATI. I quite agree with you and doubt not that, in very long distances, the lead might cover one hundred miles while the bladder was traversing one; but, my dear Simplicio, this phenomenon which you adduce against my proposition is precisely the one which confirms it. Let me once more explain that the variation of speed observed in bodies of different specific gravities is not caused by the difference of specific gravity but depends upon external circumstances and, in particular, upon the resistance of the medium, so that if this is removed all bodies would fall with the same velocity; and this result I deduce mainly from the fact which you have just admitted and which is very true, namely, that, in the case of bodies which differ widely in weight, their velocities differ more and more as the spaces traversed increase, something which would not occur if the effect depended upon differences of specific gravity. For since these specific gravities remain constant, the ratio between the distances traversed ought to remain constant whereas the fact is that this ratio keeps on increasing as the motion continues. Thus a very heavy body in a fall of one cubit will not anticipate a very light one by so much as the tenth part of this space; but in a fall of twelve cubits the heavy body would outstrip the other by one-third, and in a fall of one hundred cubits by 90/100, etc.

SIMPLICIO. Very well: but, following your own line of argument, if differences of weight in bodies of different specific gravities cannot produce a change in the ratio of their speeds, on the ground that their specific gravities do not change, how is it possible for the medium, which also we suppose to remain constant, to bring about any change in the ratio of these velocities?

SALVIATI. This objection with which you oppose my statement is clever; and I must meet it. I begin by saying that a heavy body has an inherent tendency to move with a constantly and uniformly accelerated motion toward the common center of gravity, that is, toward the center of our earth, so that during equal intervals of time it receives equal increments of momentum and velocity. This, you must understand, holds whenever all external and accidental hindrances have been removed; but of these there is one which we can never remove, namely, the medium which must be penetrated and thrust aside by the falling body. This quiet, yielding, fluid medium opposes motion through it with a resistance which is proportional to the rapidity with which the medium must give way to the passage of the body; which body, as I have said, is by nature continuously accelerated so that it meets with more and more resistance in the medium and hence a diminution in its rate of gain of speed until finally the speed reaches such a point and the resistance of the medium

becomes so great that, balancing each other, they prevent any further acceleration and reduce the motion of the body to one which is uniform and which will thereafter maintain a constant value. There is, therefore, an increase in the resistance of the medium, not on account of any change in its essential properties, but on account of the change in rapidity with which it must yield and give way laterally to the passage of the falling body which is being constantly accelerated.

Now seeing how great is the resistance which the air offers to the slight momentum of the bladder and how small that which it offers to the large weight of the lead, I am convinced that, if the medium were entirely removed, the advantage received by the bladder would be so great and that coming to the lead so small that their speeds would be equalized. Assuming this principle, that all falling bodies acquire equal speeds in a medium which, on account of a vacuum or something else, offers no resistance to the speed of the motion, we shall be able accordingly to determine the ratios of the speeds of both similar and dissimilar bodies moving either through one and the same medium or through different space-filling, and therefore resistant, media. This result we may obtain by observing how much the weight of the medium detracts from the weight of the moving body, which weight is the means employed by the falling body to open a path for itself and to push aside the parts of the medium, something which does not happen in a vacuum where, therefore, no difference [of speed] is to be expected from a difference of specific gravity. And since it is known that the effect of the medium is to diminish the weight of the body by the weight of the medium displaced, we may accomplish our purpose by diminishing in just this proportion the speeds of the falling bodies, which in a non-resisting medium we have assumed to be equal.

Thus, for example, imagine lead to be ten thousand times as heavy as air while ebony is only one thousand times as heavy. Here we have two substances whose speeds of fall in a medium devoid of resistance are equal: but, when air is the medium, it will subtract from the speed of the lead one part in ten thousand, and from the speed of the ebony one part in one thousand, i. e. ten parts in ten thousand. While therefore lead and ebony would fall from any given height in the same interval of time, provided the retarding effect of the air were removed, the lead will, in air, lose in speed one part in ten thousand; and the ebony, ten parts in ten thousand. In other words, if the elevation from which the bodies start be divided into ten thousand parts, the lead will reach the ground leaving the ebony behind by as much as ten, or at least nine, of these parts. Is it not clear then that a leaden ball allowed to fall from a tower two hundred cubits high will outstrip an ebony ball by less than four inches? Now ebony weighs a thousand times as much as air but this inflated bladder only four times as much; therefore air diminishes the inherent and natural speed of ebony by one part in a thousand; while that of the bladder which, if free from hindrance, would be the same, experiences a diminution in air amounting to one part in four. So that

when the ebony ball, falling from the tower, has reached the earth, the bladder will have traversed only three-quarters of this distance. Lead is twelve times as heavy as water; but ivory is only twice as heavy. The speeds of these two substances which, when entirely unhindered, are equal will be diminished in water, that of lead by one part in twelve, that of ivory by half. Accordingly when the lead has fallen through eleven cubits of water the ivory will have fallen through only six. Employing this principle we shall, I believe, find a much closer agreement of experiment with our computation than with that of Aristotle.

In a similar manner we may find the ratio of the speeds of one and the same body in different fluid media, not by comparing the different resistances of the media, but by considering the excess of the specific gravity of the body above those of the media. Thus, for example, tin is one thousand times heavier than air and ten times heavier than water; hence, if we divide its unhindered speed into 1000 parts, air will rob it of one of these parts so that it will fall with a speed of 999, while in water its speed will be 900, seeing that water diminishes its weight by one part in ten while air by only one part in a thousand.

Again take a solid a little heavier than water, such as oak, a ball of which will weigh let us say 1000 drachms; suppose an equal volume of water to weigh 950, and an equal volume of air, 2; then it is clear that if the unhindered speed of the ball is 1000, its speed in air will be 998, but in water only 50, seeing that the water removes 950 of the 1000 parts which the body weighs, leaving only 50.

Such a solid would therefore move almost twenty times as fast in air as in water, since its specific gravity exceeds that of water by one part in twenty. And here we must consider the fact that only those substances which have a specific gravity greater than water can fall through it—substances which must, therefore, be hundreds of times heavier than air; hence when we try to obtain the ratio of the speed in air to that in water, we may, without appreciable error, assume that air does not, to any considerable extent, diminish the free weight and consequently the unhindered speed of such substances. Having thus easily found the excess of the weight of these substances over that of water, we can say that their speed in air is to their speed in water as their free weight is to the excess of this weight over that of water. For example, a ball of ivory weighs 20 ounces; an equal volume of water weighs 17 ounces; hence the speed of ivory in air bears to its speed in water the approximate ratio of 20:3.

SAGREDO. I have made a great step forward in this truly interesting subject upon which I have long labored in vain. In order to put these theories into practice we need only discover a method of determining the specific gravity of air with reference to water and hence with reference to other heavy substances.

SIMPLICIO. But if we find that air has levity instead of gravity what then shall we say of the foregoing discussion which, in other respects, is very clever?

SALVIATI. I should say that it was empty, vain, and trifling. But can

you doubt that air has weight when you have the clear testimony of Aristotle affirming that all the elements have weight including air, and excepting only fire? As evidence of this he cites the fact that a leather bottle weighs more when inflated than when collapsed.

SIMPLICIO. I am inclined to believe that the increase of weight observed in the inflated leather bottle or bladder arises, not from the gravity of the air, but from the many thick vapors mingled with it in these lower regions. To this I would attribute the increase of weight in the leather bottle.

SALVIATI. I would not have you say this, and much less attribute it to Aristotle; because, if speaking of the elements, he wished to persuade me by experiment that air has weight and were to say to me: "Take a leather bottle, fill it with heavy vapors and observe how its weight increases," I would reply that the bottle would weigh still more if filled with bran; and would then add that this merely proves that bran and thick vapors are heavy, but in regard to air I should still remain in the same doubt as before. However, the experiment of Aristotle is good and the proposition is true. But I cannot say as much of a certain other consideration, taken at face value; this consideration was offered by a philosopher whose name slips me; but I know I have read his argument which is that air exhibits greater gravity than levity, because it carries heavy bodies downward more easily than it does light ones upward.

SAGREDO. Fine indeed! So according to this theory air is much heavier than water, since all heavy bodies are carried downward more easily through air than through water, and all light bodies buoyed up more easily through water than through air; further there is an infinite number of heavy bodies which fall through air but ascend in water and there is an infinite number of substances which rise in water and fall in air. But, Simplicio, the question as to whether the weight of the leather bottle is owing to thick vapors or to pure air does not affect our problem which is to discover how bodies move through this vapor-laden atmosphere of ours. Returning now to the question which interests me more, I should like, for the sake of more complete and thorough knowledge of this matter, not only to be strengthened in my belief that air has weight but also to learn, if possible, how great its specific gravity is. Therefore, Salviati, if you can satisfy my curiosity on this point pray do so.

SALVIATI. The experiment with the inflated leather bottle of Aristotle proves conclusively that air possesses positive gravity and not, as some have believed, levity, a property possessed possibly by no substance whatever; for if air did possess this quality of absolute and positive levity, it should on compression exhibit greater levity and, hence, a greater tendency to rise; but experiment shows precisely the opposite.

As to the other question, namely, how to determine the specific gravity of air, I have employed the following method. I took a rather large glass bottle with a narrow neck and attached to it a leather cover, binding it tightly about the neck of the bottle: in the top of this cover I inserted and firmly fastened the valve of a leather bottle, through which

I forced into the glass bottle, by means of a syringe, a large quantity of air. And since air is easily condensed one can pump into the bottle two or three times its own volume of air. After this I took an accurate balance and weighed this bottle of compressed air with the utmost precision, adjusting the weight with fine sand. I next opened the valve and allowed the compressed air to escape; then replaced the flask upon the balance and found it perceptibly lighter: from the sand which had been used as a counterweight I now removed and laid aside as much as was necessary to again secure balance. Under these conditions there can be no doubt but that the weight of the sand thus laid aside represents the weight of the air which had been forced into the flask and had afterwards escaped. But after all this experiment tells me merely that the weight of the compressed air is the same as that of the sand removed from the balance; when however it comes to knowing certainly and definitely the weight of air as compared with that of water or any other heavy substance this I cannot hope to do without first measuring the volume of compressed air; for this measurement I have devised the two following methods.

According to the first method one takes a bottle with a narrow neck similar to the previous one; over the mouth of this bottle is slipped a leather tube which is bound tightly about the neck of the flask; the other end of this tube embraces the valve attached to the first flask and is tightly bound about it. This second flask is provided with a hole in the bottom through which an iron rod can be placed so as to open, at will, the valve above mentioned and thus permit the surplus air of the first to escape after it has once been weighed: but this second bottle must be filled with water. Having prepared everything in the manner above described, open the valve with the rod; the air will rush into the flask containing the water and will drive it through the hole at the bottom, it being clear that the volume of water thus displaced is equal to the volume of air escaped from the other vessel. Having set aside this displaced water, weigh the vessel from which the air has escaped (which is supposed to have been weighed previously while containing the compressed air), and remove the surplus of sand as described above; it is then manifest that the weight of this sand is precisely the weight of a volume of air equal to the volume of water displaced and set aside; this water we can weigh and find how many times its weight contains the weight of the removed sand, thus determining definitely how many times heavier water is than air; and we shall find, contrary to the opinion of Aristotle, that this is not 10 times, but, as our experiment shows, more nearly 400 times.

The second method is more expeditious and can be carried out with a single vessel fitted up as the first was. Here no air is added to that which the vessel naturally contains but water is forced into it without allowing any air to escape; the water thus introduced necessarily compresses the air. Having forced into the vessel as much water as possible, filling it, say, three-fourths full, which does not require any extraordinary effort, place it upon the balance and weigh it accurately; next hold the vessel mouth up, open the valve, and allow the air to escape; the volume

of the air thus escaping is precisely equal to the volume of water contained in the flask. Again weigh the vessel which will have diminished in weight on account of the escaped air; this loss in weight represents the weight of a volume of air equal to the volume of water contained in the vessel.

SIMPLICIO. No one can deny the cleverness and ingenuity of your devices; but while they appear to give complete intellectual satisfaction they confuse me in another direction. For since it is undoubtedly true that the elements when in their proper places have neither weight nor levity, I cannot understand how it is possible for that portion of air, which appeared to weigh, say, 4 drachms of sand, should really have such a weight in air as the sand which counterbalances it. It seems to me, therefore, that the experiment should be carried out, not in air, but in a medium in which the air could exhibit its property of weight if such it really has.

SALVIATI. The objection of Simplicio is certainly to the point and must therefore either be unanswerable or demand an equally clear solution. It is perfectly evident that that air which, under compression, weighed as much as the sand, loses this weight when once allowed to escape into its own element, while, indeed, the sand retains its weight. Hence for this experiment it becomes necessary to select a place where air as well as sand can gravitate; because, as has been often remarked, the medium diminishes the weight of any substance immersed in it by an amount equal to the weight of the displaced medium; so that air in air loses all its weight. If therefore this experiment is to be made with accuracy it should be performed in a vacuum where every heavy body exhibits its momentum without the slightest diminution. If then, Simplicio, we were to weigh a portion of air in a vacuum would you then be satisfied and assured of the fact?

SIMPLICIO. Yes truly: but this is to wish or ask the impossible.

SALVIATI. Your obligation will then be very great if, for your sake, I accomplish the impossible. But I do not want to sell you something which I have already given you; for in the previous experiment we weighed the air in vacuum and not in air or other medium. The fact that any fluid medium diminishes the weight of the mass immersed in it is due, Simplicio, to the resistance which this medium offers to its being opened up, driven aside, and finally lifted up. The evidence for this is seen in the readiness with which the fluid rushes to fill up any space formerly occupied by the mass; if the medium were not affected by such an immersion then it would not react against the immersed body. Tell me now, when you have a flask, in air, filled with its natural amount of air and then proceed to pump into the vessel more air, does this extra charge in any way separate or divide or change the circumambient air? Does the vessel perhaps expand so that the surrounding medium is displaced in order to give more room? Certainly not. Therefore one is able to say that this extra charge of air is not immersed in the surrounding medium for it occupies no space in it, but is, as it were, in a vacuum.

Indeed, it is really in a vacuum; for it diffuses into the vacuities which are not completely filled by the original and uncondensed air. In fact I do not see any difference between the enclosed and the surrounding media: for the surrounding medium does not press upon the enclosed medium and, *vice versa,* the enclosed medium exerts no pressure against the surrounding one; this same relationship exists in the case of any matter in a vacuum, as well as in the case of the extra charge of air compressed into the flask. The weight of this condensed air is therefore the same as that which it would have set free in a vacuum. It is true of course that the weight of the sand used as a counterpoise would be a little greater *in vacuo* than in free air. We must, then, say that the air is slightly lighter than the sand required to counterbalance it, that is to say, by an amount equal to the weight *in vacuo* of a volume of air equal to the volume of the sand.

SIMPLICIO. The previous experiments, in my opinion, left something to be desired: but now I am fully satisfied.

SALVIATI. The facts set forth by me up to this point and, in particular, the one which shows that difference of weight, even when very great, is without effect in changing the speed of falling bodies, so that as far as weight is concerned they all fall with equal speed: this idea is, I say, so new, and at first glance so remote from fact, that if we do not have the means of making it just as clear as sunlight, it had better not be mentioned; but having once allowed it to pass my lips I must neglect no experiment or argument to establish it.

SAGREDO. Not only this but also many other of your views are so far removed from the commonly accepted opinions and doctrines that if you were to publish them you would stir up a large number of antagonists; for human nature is such that men do not look with favor upon discoveries—either of truth or fallacy—in their own field, when made by others than themselves. They call him an innovator of doctrine, an unpleasant title, by which they hope to cut those knots which they cannot untie, and by subterranean mines they seek to destroy structures which patient artisans have built with customary tools. But as for ourselves who have no such thoughts, the experiments and arguments which you have thus far adduced are fully satisfactory; however if you have any experiments which are more direct or any arguments which are more convincing we will hear them with pleasure.

SALVIATI. The experiment made to ascertain whether two bodies, differing greatly in weight, will fall from a given height with the same speed offers some difficulty; because, if the height is considerable, the retarding effect of the medium, which must be penetrated and thrust aside by the falling body, will be greater in the case of the small momentum of the very light body than in the case of the great force of the heavy body; so that, in a long distance, the light body will be left behind; if the height be small, one may well doubt whether there is any difference; and if there be a difference it will be inappreciable.

It occurred to me therefore to repeat many times the fall through

a small height in such a way that I might accumulate all those small intervals of time that elapse between the arrival of the heavy and light bodies respectively at their common terminus, so that this sum makes an interval of time which is not only observable, but easily observable. In order to employ the slowest speeds possible and thus reduce the change which the resisting medium produces upon the simple effect of gravity it occurred to me to allow the bodies to fall along a plane slightly inclined to the horizontal. For in such a plane, just as well as in a vertical plane, one may discover how bodies of different weight behave: and besides this, I also wished to rid myself of the resistance which might arise from contact of the moving body with the aforesaid inclined plane. Accordingly I took two balls, one of lead and one of cork, the former more than a hundred times heavier than the latter, and suspended them by means of two equal fine threads, each four or five cubits long. Pulling each ball aside from the perpendicular, I let them go at the same instant, and they, falling along the circumferences of circles having these equal strings for semi-diameters, passed beyond the perpendicular and returned along the same path. This free vibration repeated a hundred times showed clearly that the heavy body maintains so nearly the period of the light body that neither in a hundred swings nor even in a thousand will the former anticipate the latter by as much as a single moment, so perfectly do they keep step. We can also observe the effect of the medium which, by the resistance which it offers to motion, diminishes the vibration of the cork more than that of the lead, but without altering the frequency of either; even when the arc traversed by the cork did not exceed five or six degrees while that of the lead was fifty or sixty, the swings were performed in equal times.

SIMPLICIO. If this be so, why is not the speed of the lead greater than that of the cork, seeing that the former traverses sixty degrees in the same interval in which the latter covers scarcely six?

SALVIATI. But what would you say, Simplicio, if both covered their paths in the same time when the cork, drawn aside through thirty degrees, traverses an arc of sixty, while the lead pulled aside only two degrees traverses an arc of four? Would not then the cork be proportionately swifter? And yet such is the experimental fact. But observe this: having pulled aside the pendulum of lead, say through an arc of fifty degrees, and set it free, it swings beyond the perpendicular almost fifty degrees, thus describing an arc of nearly one hundred degrees; on the return swing it describes a little smaller arc; and after a large number of such vibrations it finally comes to rest. Each vibration, whether of ninety, fifty, twenty, ten, or four degrees, occupies the same time: accordingly the speed of the moving body keeps on diminishing since in equal intervals of time it traverses arcs which grow smaller and smaller.

Precisely the same things happen with the pendulum of cork, suspended by a string of equal length, except that a smaller number of vibrations is required to bring it to rest, since on account of its lightness it is less able to overcome the resistance of the air; nevertheless the vibra-

tions, whether large or small, are all performed in time-intervals which are not only equal among themselves, but also equal to the period of the lead pendulum. Hence it is true that, if while the lead is traversing an arc of fifty degrees the cork covers one of only ten, the cork moves more slowly than the lead; but on the other hand it is also true that the cork may cover an arc of fifty while the lead passes over one of only ten or six; thus, at different times, we have now the cork, now the lead, moving more rapidly. But if these same bodies traverse equal arcs in equal times we may rest assured that their speeds are equal.

SIMPLICIO. I hesitate to admit the conclusiveness of this argument because of the confusion which arises from your making both bodies move now rapidly, now slowly and now very slowly, which leaves me in doubt as to whether their velocities are always equal.

SAGREDO. Allow me, if you please, Salviati, to say just a few words. Now tell me, Simplicio, whether you admit that one can say with certainty that the speeds of the cork and the lead are equal whenever both, starting from rest at the same moment and descending the same slopes, always traverse equal spaces in equal times?

SIMPLICIO. This can neither be doubted nor gainsaid.

SAGREDO. Now it happens, in the case of the pendulums, that each of them traverses now an arc of sixty degrees, now one of fifty, or thirty or ten or eight or four or two, etc.; and when they both swing through an arc of sixty degrees they do so in equal intervals of time; the same thing happens when the arc is fifty degrees or thirty or ten or any other number; and therefore we conclude that the speed of the lead in an arc of sixty degrees is equal to the speed of the cork when the latter also swings through an arc of sixty degrees; in the case of a fifty-degree arc these speeds are also equal to each other; so also in the case of other arcs. But this is not saying that the speed which occurs in an arc of sixty is the same as that which occurs in an arc of fifty; nor is the speed in an arc of fifty equal to that in one of thirty, etc.; but the smaller the arcs, the smaller the speeds; the fact observed is that one and the same moving body requires the same time for traversing a large arc of sixty degrees as for a small arc of fifty or even a very small arc of ten; all these arcs, indeed, are covered in the same interval of time. It is true therefore that the lead and the cork each diminish their speed in proportion as their arcs diminish; but this does not contradict the fact that they maintain equal speeds in equal arcs.

My reason for saying these things has been rather because I wanted to learn whether I had correctly understood Salviati, than because I thought Simplicio had any need of a clearer explanation than that given by Salviati which like everything else of his is extremely lucid, so lucid, indeed, that when he solves questions which are difficult not merely in appearance, but in reality and in fact, he does so with reasons, observations and experiments which are common and familiar to everyone.

In this manner he has, as I have learned from various sources, given occasion to a highly esteemed professor for undervaluing his discoveries

on the ground that they are commonplace, and established upon a mean and vulgar basis; as if it were not a most admirable and praiseworthy feature of demonstrative science that it springs from and grows out of principles well-known, understood and conceded by all.

But let us continue with this light diet; and if Simplicio is satisfied to understand and admit that the gravity inherent in various falling bodies has nothing to do with the difference of speed observed among them, and that all bodies, in so far as their speeds depend upon it, would move with the same velocity, pray tell us, Salviati, how you explain the appreciable and evident inequality of motion; please reply also to the objection urged by Simplicio—an objection in which I concur—namely, that a cannon ball falls more rapidly than a bird shot. From my point of view, one might expect the difference of speed to be small in the case of bodies of the same substance moving through any single medium, whereas the larger ones will descend, during a single pulse beat, a distance which the smaller ones will not traverse in an hour, or in four, or even in twenty hours; as for instance in the case of stones and fine sand and especially that very fine sand which produces muddy water and which in many hours will not fall through as much as two cubits, a distance which stones not much larger will traverse in a single pulse beat.

SALVIATI. The action of the medium in producing a greater retardation upon those bodies which have a less specific gravity has already been explained by showing that they experience a diminution of weight. But to explain how one and the same medium produces such different retardations in bodies which are made of the same material and have the same shape, but differ only in size, requires a discussion more clever than that by which one explains how a more expanded shape or an opposing motion of the medium retards the speed of the moving body. The solution of the present problem lies, I think, in the roughness and porosity which are generally and almost necessarily found in the surfaces of solid bodies. When the body is in motion these rough places strike the air or other ambient medium. The evidence for this is found in the humming which accompanies the rapid motion of a body through air, even when that body is as round as possible. One hears not only humming, but also hissing and whistling, whenever there is any appreciable cavity or elevation upon the body. We observe also that a round solid body rotating in a lathe produces a current of air. But what more do we need? When a top spins on the ground at its greatest speed do we not hear a distinct buzzing of high pitch? This sibilant note diminishes in pitch as the speed of rotation slackens, which is evidence that these small rugosities on the surface meet resistance in the air. There can be no doubt, therefore, that in the motion of falling bodies these rugosities strike the surrounding fluid and retard the speed; and this they do so much the more in proportion as the surface is larger, which is the case of small bodies as compared with greater.

SIMPLICIO. Stop a moment please, I am getting confused. For although I understand and admit that friction of the medium upon the

surface of the body retards its motion and that, if other things are the same, the larger surface suffers greater retardation, I do not see on what ground you say that the surface of the smaller body is larger. Besides if, as you say, the larger surface suffers greater retardation the larger solid should move more slowly, which is not the fact. But this objection can be easily met by saying that, although the larger body has a larger surface, it has also a greater weight, in comparison with which the resistance of the larger surface is no more than the resistance of the small surface in comparison with its smaller weight; so that the speed of the larger solid does not become less. I therefore see no reason for expecting any difference of speed so long as the driving weight diminishes in the same proportion as the retarding power of the surface.

SALVIATI. I shall answer all your objections at once. You will admit, of course, Simplicio, that if one takes two equal bodies, of the same material and same figure, bodies which would therefore fall with equal speeds, and if he diminishes the weight of one of them in the same proportion as its surface (maintaining the similarity of shape) he would not thereby diminish the speed of this body.

SIMPLICIO. This inference seems to be in harmony with your theory which states that the weight of a body has no effect in either accelerating or retarding its motion.

SALVIATI. I quite agree with you in this opinion from which it appears to follow that, if the weight of a body is diminished in greater proportion than its surface, the motion is retarded to a certain extent; and this retardation is greater and greater in proportion as the diminution of weight exceeds that of the surface.

SIMPLICIO. This I admit without hesitation.

SALVIATI. Now you must know, Simplicio, that it is not possible to diminish the surface of a solid body in the same ratio as the weight, and at the same time maintain similarity of figure. For since it is clear that in the case of a diminishing solid the weight grows less in proportion to the volume, and since the volume always diminishes more rapidly than the surface, when the same shape is maintained, the weight must therefore diminish more rapidly than the surface. But geometry teaches us that, in the case of similar solids, the ratio of two volumes is greater than the ratio of their surfaces; which, for the sake of better understanding, I shall illustrate by a particular case.

Take, for example, a cube two inches on a side so that each face has an area of four square inches and the total area, i. e., the sum of the six faces, amounts to twenty-four square inches; now imagine this cube to be sawed through three times so as to divide it into eight smaller cubes, each one inch on the side, each face one inch square, and the total surface of each cube six square inches instead of twenty-four as in the case of the larger cube. It is evident therefore that the surface of the little cube is only one-fourth that of the larger, namely, the ratio of six to twenty-four; but the volume of the solid cube itself is only one-eighth; the volume, and hence also the weight, diminishes therefore much more rapidly

than the surface. If we again divide the little cube into eight others we shall have, for the total surface of one of these, one and one-half square inches, which is one-sixteenth of the surface of the original cube; but its volume is only one-sixty-fourth part. Thus, by two divisions, you see that the volume is diminished four times as much as the surface. And, if the subdivision be continued until the original solid be reduced to a fine powder, we shall find that the weight of one of these smallest particles has diminished hundreds and hundreds of times as much as its surface. And this which I have illustrated in the case of cubes holds also in the case of all similar solids. Observe then how much greater the resistance, arising from contact of the surface of the moving body with the medium, in the case of small bodies than in the case of large; and when one considers that the rugosities on the very small surfaces of fine dust particles are perhaps no smaller than those on the surfaces of larger solids which have been carefully polished, he will see how important it is that the medium should be very fluid and offer no resistance to being thrust aside, easily yielding to a small force. You see, therefore, Simplicio, that I was not mistaken when, not long ago, I said that the surface of a small solid is comparatively greater than that of a large one.

SIMPLICIO. I am quite convinced; and, believe me, if I were again beginning my studies, I should follow the advice of Plato and start with mathematics, a science which proceeds very cautiously and admits nothing as established until it has been rigidly demonstrated.

SAGREDO. This discussion has afforded me great pleasure. And now although there are still some details, in connection with the subject under discussion, concerning which I might ask questions yet, if we keep making one digression after another, it will be long before we reach the main topic which has to do with the variety of properties found in the resistance which solid bodies offer to fracture; and, therefore, if you please, let us return to the subject which we originally proposed to discuss.

SALVIATI. Very well; but the questions which we have already considered are so numerous and so varied, and have taken up so much time, that there is not much of this day left to spend upon our main topic which abounds in geometrical demonstrations calling for careful consideration. May I, therefore, suggest that we postpone the meeting until tomorrow, not only for the reason just mentioned but also in order that I may bring with me some papers in which I have set down in an orderly way the theorems and propositions dealing with the various phases of this subject, matters which, from memory alone, I could not present in the proper order.

SAGREDO. I fully concur in your opinion and all the more willingly because this will leave time today to take up some of my difficulties with the subject which we have just been discussing. One question is whether we are to consider the resistance of the medium as sufficient to destroy the acceleration of a body of very heavy material, very large volume, and spherical figure. I say *spherical* in order to select a volume which is con-

tained within a minimum surface and therefore less subject to retardation.

Another question deals with the vibrations of pendulums which may be regarded from several viewpoints; the first is whether all vibrations, large, medium, and small, are performed in exactly and precisely equal times: another is to find the ratio of the times of vibration of pendulums supported by threads of unequal length.

SALVIATI. These are interesting questions: but I fear that here, as in the case of all other facts, if we take up for discussion any one of them, it will carry in its wake so many other facts and curious consequences that time will not remain today for the discussion of all.

SAGREDO. If these are as full of interest as the foregoing, I would gladly spend as many days as there remain hours between now and nightfall; and I dare say that Simplicio would not be wearied by these discussions.

SIMPLICIO. Certainly not; especially when the questions pertain to natural science and have not been treated by other philosophers.

SALVIATI. Now taking up the first question, I can assert without hesitation that there is no sphere so large, or composed of material so dense, but that the resistance of the medium, although very slight, would check its acceleration and would, in time, reduce its motion to uniformity; a statement which is strongly supported by experiment. For if a falling body, as time goes on, were to acquire a speed as great as you please, no such speed, impressed by external forces, can be so great but that the body will first acquire it and then, owing to the resisting medium, lose it. Thus, for instance, if a cannon ball, having fallen a distance of four cubits through the air and having acquired a speed of, say, ten units were to strike the surface of the water, and if the resistance of the water were not able to check the momentum of the shot, it would either increase in speed or maintain a uniform motion until the bottom were reached: but such is not the observed fact; on the contrary, the water when only a few cubits deep hinders and diminishes the motion in such a way that the shot delivers to the bed of the river or lake a very slight impulse. Clearly then if a short fall through the water is sufficient to deprive a cannon ball of its speed, this speed cannot be regained by a fall of even a thousand cubits. How could a body acquire, in a fall of a thousand cubits, that which it loses in a fall of four? But what more is needed? Do we not observe that the enormous momentum, delivered to a shot by a cannon, is so deadened by passing through a few cubits of water that the ball, so far from injuring the ship, barely strikes it? Even the air, although a very yielding medium, can also diminish the speed of a falling body, as may be easily understood from similar experiments. For if a gun be fired downwards from the top of a very high tower the shot will make a smaller impression upon the ground than if the gun had been fired from an elevation of only four or six cubits; this is clear evidence that the momentum of the ball, fired from the top of the tower, diminishes continually from the instant it leaves the barrel until it reaches the ground.

Therefore a fall from ever so great an altitude will not suffice to give to a body that momentum which it has once lost through the resistance of the air, no matter how it was originally acquired. In like manner, the destructive effect produced upon a wall by a shot fired from a gun at a distance of twenty cubits cannot be duplicated by the fall of the same shot from any altitude however great. My opinion is, therefore, that under the circumstances which occur in nature, the acceleration of any body falling from rest reaches an end and that the resistance of the medium finally reduces its speed to a constant value which is thereafter maintained.

SAGREDO. These experiments are in my opinion much to the purpose; the only question is whether an opponent might not make bold to deny the fact in the case of bodies which are very large and heavy or to assert that a cannon ball, falling from the distance of the moon or from the upper regions of the atmosphere, would deliver a heavier blow than if just leaving the muzzle of the gun.

SALVIATI. No doubt many objections may be raised not all of which can be refuted by experiment: however in this particular case the following consideration must be taken into account, namely, that it is very likely that a heavy body falling from a height will, on reaching the ground, have acquired just as much momentum as was necessary to carry it to that height; as may be clearly seen in the case of a rather heavy pendulum which, when pulled aside fifty or sixty degrees from the vertical, will acquire precisely that speed and force which are sufficient to carry it to an equal elevation save only that small portion which it loses through friction on the air. In order to place a cannon ball at such a height as might suffice to give it just that momentum which the powder imparted to it on leaving the gun we need only fire it vertically upwards from the same gun; and we can then observe whether on falling back it delivers a blow equal to that of the gun fired at close range; in my opinion it would be much weaker. The resistance of the air would, therefore, I think, prevent the muzzle velocity from being equalled by a natural fall from rest at any height whatsoever.

We come now to the other questions, relating to pendulums, a subject which may appear to many exceedingly arid, especially to those philosophers who are continually occupied with the more profound questions of nature. Nevertheless, the problem is one which I do not scorn. I am encouraged by the example of Aristotle whom I admire especially because he did not fail to discuss every subject which he thought in any degree worthy of consideration.

Impelled by your queries I may give you some of my ideas concerning certain problems in music, a splendid subject, upon which so many eminent men have written: among these is Aristotle himself who has discussed numerous interesting acoustical questions. Accordingly, if on the basis of some easy and tangible experiments, I shall explain some striking phenomena in the domain of sound, I trust my explanations will meet your approval.

SAGREDO. I shall receive them not only gratefully but eagerly. For,

although I take pleasure in every kind of musical instrument and have paid considerable attention to harmony, I have never been able to fully understand why some combinations of tones are more pleasing than others, or why certain combinations not only fail to please but are even highly offensive. Then there is the old problem of two stretched strings in unison; when one of them is sounded, the other begins to vibrate and to emit its note; nor do I understand the different ratios of harmony and some other details.

SALVIATI. Let us see whether we cannot derive from the pendulum a satisfactory solution of all these difficulties. And first, as to the question whether one and the same pendulum really performs its vibrations, large, medium, and small, all in exactly the same time, I shall rely upon what I have already heard from our Academician. He has clearly shown that the time of descent is the same along all chords, whatever the arcs which subtend them, as well along an arc of 180° (i. e., the whole diameter) as along one of 100°, 60°, 10°, 2°, ½°, or 4′. It is understood, of course, that these arcs all terminate at the lowest point of the circle, where it touches the horizontal plane.

If now we consider descent along arcs instead of their chords then, provided these do not exceed 90°, experiment shows that they are all traversed in equal times; but these times are greater for the chord than for the arc, an effect which is all the more remarkable because at first glance one would think just the opposite to be true. For since the terminal points of the two motions are the same and since the straight line included between these two points is the shortest distance between them, it would seem reasonable that motion along this line should be executed in the shortest time; but this is not the case, for the shortest time—and therefore the most rapid motion—is that employed along the arc of which this straight line is the chord.

As to the times of vibration of bodies suspended by threads of different lengths, they bear to each other the same proportion as the square roots of the lengths of the thread; or one might say the lengths are to each other as the squares of the times; so that if one wishes to make the vibration time of one pendulum twice that of another, he must make its suspension four times as long. In like manner, if one pendulum has a suspension nine times as long as another, this second pendulum will execute three vibrations during each one of the first; from which it follows that the lengths of the suspending cords bear to each other the [inverse] ratio of the squares of the number of vibrations performed in the same time.

SAGREDO. Then, if I understand you correctly, I can easily measure the length of a string whose upper end is attached at any height whatever even if this end were invisible and I could see only the lower extremity. For if I attach to the lower end of this string a rather heavy weight and give it a to-and-fro motion, and if I ask a friend to count a number of its vibrations, while I, during the same time-interval, count the number of vibrations of a pendulum which is exactly one cubit in length, then knowing the number of vibrations which each pendulum makes in the

given interval of time one can determine the length of the string. Suppose, for example, that my friend counts 20 vibrations of the long cord during the same time in which I count 240 of my string which is one cubit in length; taking the squares of the two numbers, 20 and 240, namely 400 and 57600, then, I say, the long string contains 57600 units of such length that my pendulum will contain 400 of them; and since the length of my string is one cubit, I shall divide 57600 by 400 and thus obtain 144. Accordingly I shall call the length of the string 144 cubits.

SALVIATI. Nor will you miss it by as much as a handsbreadth, especially if you observe a large number of vibrations.

SAGREDO. You give me frequent occasion to admire the wealth and profusion of nature when, from such common and even trivial phenomena, you derive facts which are not only striking and new but which are often far removed from what we would have imagined. Thousands of times I have observed vibrations especially in churches where lamps, suspended by long cords, had been inadvertently set into motion; but the most which I could infer from these observations was that the view of those who think that such vibrations are maintained by the medium is highly improbable: for, in that case, the air must needs have considerable judgment and little else to do but kill time by pushing to and fro a pendent weight with perfect regularity. But I never dreamed of learning that one and the same body, when suspended from a string a hundred cubits long and pulled aside through an arc of 90° or even 1° or ½°, would employ the same time in passing through the least as through the largest of these arcs; and, indeed, it still strikes me as somewhat unlikely. Now I am waiting to hear how these same simple phenomena can furnish solutions for those acoustical problems—solutions which will be at least partly satisfactory.

SALVIATI. First of all one must observe that each pendulum has its own time of vibration so definite and determinate that it is not possible to make it move with any other period than that which nature has given it. For let any one take in his hand the cord to which the weight is attached and try, as much as he pleases, to increase or diminish the frequency of its vibrations; it will be time wasted. On the other hand, one can confer motion upon even a heavy pendulum which is at rest by simply blowing against it; by repeating these blasts with a frequency which is the same as that of the pendulum one can impart considerable motion. Suppose that by the first puff we have displaced the pendulum from the vertical by, say, half an inch; then if, after the pendulum has returned and is about to begin the second vibration, we add a second puff, we shall impart additional motion; and so on with other blasts provided they are applied at the right instant, and not when the pendulum is coming toward us since in this case the blast would impede rather than aid the motion. Continuing thus with many impulses we impart to the pendulum such momentum that a greater impulse than that of a single blast will be needed to stop it.

SAGREDO. Even as a boy, I observed that one man alone by giving these impulses at the right instant was able to ring a bell so large that when

four, or even six, men seized the rope and tried to stop it they were lifted from the ground, all of them together being unable to counterbalance the momentum which a single man, by properly timed pulls, had given it.

SALVIATI. Your illustration makes my meaning clear and is quite as well fitted, as what I have just said, to explain the wonderful phenomenon of the strings of the cittern or of the spinet, namely, the fact that a vibrating string will set another string in motion and cause it to sound not only when the latter is in unison but even when it differs from the former by an octave or a fifth. A string which has been struck begins to vibrate and continues the motion as long as one hears the sound; these vibrations cause the immediately surrounding air to vibrate and quiver; then these ripples in the air expand far into space and strike not only all the strings of the same instrument but even those of neighboring instruments. Since that string which is tuned to unison with the one plucked is capable of vibrating with the same frequency, it acquires, at the first impulse, a slight oscillation; after receiving two, three, twenty, or more impulses, delivered at proper intervals, it finally accumulates a vibratory motion equal to that of the plucked string, as is clearly shown by equality of amplitude in their vibrations. This undulation expands through the air and sets into vibration not only strings, but also any other body which happens to have the same period as that of the plucked string. Accordingly if we attach to the side of an instrument small pieces of bristle or other flexible bodies, we shall observe that, when a spinet is sounded, only those pieces respond that have the same period as the string which has been struck; the remaining pieces do not vibrate in response to this string, nor do the former pieces respond to any other tone.

If one bows the bass string on a viola rather smartly and brings near it a goblet of fine, thin glass having the same tone as that of the string, this goblet will vibrate and audibly resound. That the undulations of the medium are widely dispersed about the sounding body is evinced by the fact that a glass of water may be made to emit a tone merely by the friction of the finger tip upon the rim of the glass; for in this water is produced a series of regular waves. The same phenomenon is observed to better advantage by fixing the base of the goblet upon the bottom of a rather large vessel of water filled nearly to the edge of the goblet; for if, as before, we sound the glass by friction of the finger, we shall see ripples spreading with the utmost regularity and with high speed to large distances about the glass. I have often remarked, in thus sounding a rather large glass nearly full of water, that at first the waves are spaced with great uniformity, and when, as sometimes happens, the tone of the glass jumps an octave higher I have noted that at this moment each of the aforesaid waves divides into two; a phenomenon which shows clearly that the ratio involved in the octave is two.

SAGREDO. More than once have I observed this same thing, much to my delight and also to my profit. For a long time I have been perplexed about these different harmonies since the explanations hitherto given by those learned in music impress me as not sufficiently conclusive. They tell

us that the diapason, i. e., the octave, involves the ratio of two, that the diapente which we call the fifth involves a ratio of 3:2, etc.; because if the open string of a monochord be sounded and afterwards a bridge be placed in the middle and the half length be sounded one hears the octave; and if the bridge be placed at 1/3 the length of the string, then on plucking first the open string and afterwards 2/3 of its length the fifth is given; for this reason they say that the octave depends upon the ratio of two to one and the fifth upon the ratio of three to two. This explanation does not impress me as sufficient to establish 2 and 3/2 as the natural ratios of the octave and the fifth; and my reason for thinking so is as follows. There are three different ways in which the tone of a string may be sharpened, namely, by shortening it, by stretching it, and by making it thinner. If the tension and size of the string remain constant one obtains the octave by shortening it to one-half, i. e., by sounding first the open string and then one-half of it; but if length and size remain constant and one attempts to produce the octave by stretching he will find that it does not suffice to double the stretching weight; it must be quadrupled; so that, if the fundamental note is produced by a weight of one pound, four will be required to bring out the octave.

And finally if the length and tension remain constant, while one changes the size of the string he will find that in order to produce the octave the size must be reduced to ¼ that which gave the fundamental. And what I have said concerning the octave, namely, that its ratio as derived from the tension and size of the string is the square of that derived from the length, applies equally well to all other musical intervals.

Thus if one wishes to produce a fifth by changing the length he finds that the ratio of the lengths must be sesquialteral, in other words he sounds first the open string, then two-thirds of it; but if he wishes to produce this same result by stretching or thinning the string then it becomes necessary to square the ratio 3/2 that is by taking 9/4; accordingly, if the fundamental requires a weight of 4 pounds, the higher note will be produced not by 6, but by 9 pounds; the same is true in regard to size, the string which gives the fundamental is larger than that which yields the fifth in the ratio of 9 to 4.

In view of these facts, I see no reason why those wise philosophers should adopt 2 rather than 4 as the ratio of the octave, or why in the case of the fifth they should employ the sesquialteral ratio, 3/2, rather than that of 9/4. Since it is impossible to count the vibrations of a sounding string on account of its high frequency, I should still have been in doubt as to whether a string, emitting the upper octave, made twice as many vibrations in the same time as one giving the fundamental, had it not been for the following fact, namely, that at the instant when the tone jumps to the octave, the waves which constantly accompany the vibrating glass divide up into smaller ones which are precisely half as long as the former.

SALVIATI. This is a beautiful experiment enabling us to distinguish individually the waves which are produced by the vibrations of a sonorous

body, which spread through the air, bringing to the tympanum of the ear a stimulus which the mind translates into sound. But since these waves in the water last only so long as the friction of the finger continues and are, even then, not constant but are always forming and disappearing, would it not be a fine thing if one had the ability to produce waves which would persist for a long while, even months and years, so as to easily measure and count them?

SAGREDO. Such an invention would, I assure you, command my admiration.

SALVIATI. The device is one which I hit upon by accident; my part consists merely in the observation of it and in the appreciation of its value as a confirmation of something to which I had given profound consideration; and yet the device is, in itself, rather common. As I was scraping a brass plate with a sharp iron chisel in order to remove some spots from it and was running the chisel rather rapidly over it, I once or twice, during many strokes, heard the plate emit a rather strong and clear whistling sound; on looking at the plate more carefully, I noticed a long row of fine streaks parallel and equidistant from one another. Scraping with the chisel over and over again, I noticed that it was only when the plate emitted this hissing noise that any marks were left upon it; when the scraping was not accompanied by this sibilant note there was not the least trace of such marks. Repeating the trick several times and making the stroke, now with greater now with less speed, the whistling followed with a pitch which was correspondingly higher and lower. I noted also that the marks made when the tones were higher were closer together; but when the tones were deeper, they were farther apart. I also observed that when, during a single stroke, the speed increased toward the end the sound became sharper and the streaks grew closer together, but always in such a way as to remain sharply defined and equidistant. Besides whenever the stroke was accompanied by hissing I felt the chisel tremble in my grasp and a sort of shiver run through my hand. In short we see and hear in the case of the chisel precisely that which is seen and heard in the case of a whisper followed by a loud voice; for, when the breath is emitted without the production of a tone, one does not feel either in the throat or mouth any motion to speak of in comparison with that which is felt in the larynx and upper part of the throat when the voice is used, especially when the tones employed are low and strong.

At times I have also observed among the strings of the spinet two which were in unison with two of the tones produced by the aforesaid scraping; and among those which differed most in pitch I found two which were separated by an interval of a perfect fifth. Upon measuring the distance between the markings produced by the two scrapings it was found that the space which contained 45 of one contained 30 of the other, which is precisely the ratio assigned to the fifth.

But now before proceeding any farther I want to call your attention to the fact that, of the three methods for sharpening a tone, the one which you refer to as the fineness of the string should be attributed to its weight.

So long as the material of the string is unchanged, the size and weight vary in the same ratio. Thus in the case of gut strings, we obtain the octave by making one string 4 times as large as the other; so also in the case of brass one wire must have 4 times the size of the other; but if now we wish to obtain the octave of a gut string, by use of brass wire, we must make it, not four times as large, but four times as heavy as the gut string: as regards size therefore the metal string is not four times as big but four times as heavy. The wire may therefore be even thinner than the gut notwithstanding the fact that the latter gives the higher note. Hence if two spinets are strung, one with gold wire the other with brass, and if the corresponding strings each have the same length, diameter, and tension it follows that the instrument strung with gold will have a pitch about one-fifth lower than the other because gold has a density almost twice that of brass. And here it is to be noted that it is the weight rather than the size of a moving body which offers resistance to change of motion contrary to what one might at first glance think. For it seems reasonable to believe that a body which is large and light should suffer greater retardation of motion in thrusting aside the medium than would one which is thin and heavy; yet here exactly the opposite is true.

Returning now to the original subject of discussion, I assert that the ratio of a musical interval is not immediately determined either by the length, size, or tension of the strings but rather by the ratio of their frequencies, that is, by the number of pulses of air waves which strike the tympanum of the ear, causing it also to vibrate with the same frequency. This fact established, we may possibly explain why certain pairs of notes, differing in pitch, produce a pleasing sensation, others a less pleasant effect, and still others a disagreeable sensation. Such an explanation would be tantamount to an explanation of the more or less perfect consonances and of dissonances. The unpleasant sensation produced by the latter arises, I think, from the discordant vibrations of two different tones which strike the ear out of time. Especially harsh is the dissonance between notes whose frequencies are incommensurable; such a case occurs when one has two strings in unison and sounds one of them open, together with a part of the other which bears the same ratio to its whole length as the side of a square bears to the diagonal; this yields a dissonance similar to the augmented fourth or diminished fifth.

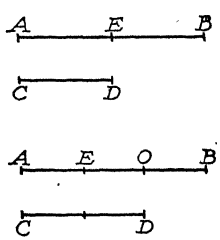
Agreeable consonances are pairs of tones which strike the ear with a certain regularity; this regularity consists in the fact that the pulses delivered by the two tones, in the same interval of time, shall be commensurable in number, so as not to keep the eardrum in perpetual torment, bending in two different directions in order to yield to the ever-discordant impulses.

The first and most pleasing consonance is, therefore, the octave since, for every pulse given to the tympanum by the lower string, the sharp string delivers two; accordingly at every other vibration of the upper string both pulses are delivered simultaneously so that one-half the entire number of pulses are delivered in unison. But when two strings are in

unison their vibrations always coincide and the effect is that of a single string; hence we do not refer to it as consonance. The fifth is also a pleasing interval since for every two vibrations of the lower string the upper one gives three, so that considering the entire number of pulses from the upper string one-third of them will strike in unison, i. e., between each pair of concordant vibrations there intervene two single vibrations; and when the interval is a fourth, three single vibrations intervene. In case the interval is a second where the ratio is 9/8 it is only every ninth vibration of the upper string which reaches the ear simultaneously with one of the lower; all the others are discordant and produce a harsh effect upon the recipient ear which interprets them as dissonances.

SIMPLICIO. Won't you be good enough to explain this argument a little more clearly?

SALVIATI. Let AB denote the length of a wave emitted by the lower string and CD that of a higher string which is emitting the octave of AB; divide AB in the middle at E. If the two strings begin their motions at A



and C, it is clear that when the sharp vibration has reached the end D, the other vibration will have travelled only as far as E, which, not being a terminal point, will emit no pulse; but there is a blow delivered at D. Accordingly when the one wave comes back from D to C, the other passes on from E to B; hence the two pulses from B and C strike the drum of the ear simultaneously. Seeing that these vibrations are repeated again and again in the same manner, we conclude that each alternate pulse from CD falls in unison with one from AB. But each of the pulsations at the terminal points, A and B, is constantly accompanied by one which leaves always from C or always from D. This is clear because if we suppose the waves to reach A and C at the same instant, then, while one wave travels from A to B, the other will proceed from C to D and back to C, so that waves strike at C and B simultaneously; during the passage of the wave from B back to A the disturbance at C goes to D and again returns to C, so that once more the pulses at A and C are simultaneous.

Next let the vibrations AB and CD be separated by an interval of a fifth, that is, by a ratio of 3/2; choose the points E and O such that they will divide the wave length of the lower string into three equal parts and imagine the vibrations to start at the same instant from each of the terminals A and C. It is evident that when the pulse has been delivered at the

terminal D, the wave in AB has travelled only as far as O; the drum of the ear receives, therefore, only the pulse from D. Then during the return of the one vibration from D to C, the other will pass from O to B and then back to O, producing an isolated pulse at B—a pulse which is out of time but one which must be taken into consideration.

Now since we have assumed that the first pulsations started from the terminals A and C at the same instant, it follows that the second pulsation, isolated at D, occurred after an interval of time equal to that required for passage from C to D or, what is the same thing, from A to O; but the next pulsation, the one at B, is separated from the preceding by only half this interval, namely, the time required for passage from O to B. Next while the one vibration travels from O to A, the other travels from C to D, the result of which is that two pulsations occur simultaneously at A and D. Cycles of this kind follow one after another, i. e., one solitary pulse of the lower string interposed between two solitary pulses of the upper string. Let us now imagine time to be divided into very small equal intervals; then if we assume that, during the first two of these intervals, the disturbances which occurred simultaneously at A and C have travelled as far as O and D and have produced a pulse at D; and if we assume that during the third and fourth intervals one disturbance returns from D to C, producing a pulse at C, while the other, passing on from O to B and back to O, produces a pulse at B; and if finally, during the fifth and sixth intervals, the disturbances travel from O and C to A and D, producing a pulse at each of the latter two, then the sequence in which the pulses strike the ear will be such that, if we begin to count time from any instant where two pulses are simultaneous, the eardrum will, after the lapse of two of the said intervals, receive a solitary pulse; at the end of the third interval, another solitary pulse; so also at the end of the fourth interval; and two intervals later, i. e., at the end of the sixth interval, will be heard two pulses in unison. Here ends the cycle—the anomaly, so to speak—which repeats itself over and over again.

SAGREDO. I can no longer remain silent; for I must express to you the great pleasure I have in hearing such a complete explanation of phenomena with regard to which I have so long been in darkness. Now I understand why unison does not differ from a single tone; I understand why the octave is the principal harmony, but so like unison as often to be mistaken for it and also why it occurs with the other harmonies. It resembles unison because the pulsations of strings in unison always occur simultaneously, and those of the lower string of the octave are always accompanied by those of the upper string; and among the latter is interposed a solitary pulse at equal intervals and in such a manner as to produce no disturbance; the result is that such a harmony is rather too much softened and lacks fire. But the fifth is characterized by its displaced beats and by the interposition of two solitary beats of the upper string and one solitary beat of the lower string between each pair of simultaneous pulses; these three solitary pulses are separated by intervals of time equal to half the interval which separates each pair of simultaneous beats from the solitary

beats of the upper string. Thus the effect of the fifth is to produce a tickling of the eardrum such that its softness is modified with sprightliness, giving at the same moment the impression of a gentle kiss and of a bite.

SALVIATI. Seeing that you have derived so much pleasure from these novelties, I must show you a method by which the eye may enjoy the same game as the ear. Suspend three balls of lead, or other heavy material, by means of strings of different length such that while the longest makes two vibrations the shortest will make four and the medium three; this will take place when the longest string measures 16, either in handbreadths or in any other unit, the medium 9 and the shortest 4, all measured in the same unit.

Now pull all these pendulums aside from the perpendicular and release them at the same instant; you will see a curious interplay of the threads passing each other in various manners but such that at the completion of every fourth vibration of the longest pendulum, all three will arrive simultaneously at the same terminus, whence they start over again to repeat the same cycle. This combination of vibrations, when produced on strings, is precisely that which yields the interval of the octave and the intermediate fifth. If we employ the same disposition of apparatus but change the lengths of the threads, always however in such a way that their vibrations correspond to those of agreeable musical intervals, we shall see a different crossing of these threads but always such that, after a definite interval of time and after a definite number of vibrations, all the threads, whether three or four, will reach the same terminus at the same instant, and then begin a repetition of the cycle.

If however the vibrations of two or more strings are incommensurable so that they never complete a definite number of vibrations at the same instant, or if commensurable they return only after a long interval of time and after a large number of vibrations, then the eye is confused by the disorderly succession of crossed threads. In like manner the ear is pained by an irregular sequence of air waves which strike the tympanum without any fixed order.

But, gentlemen, whither have we drifted during these many hours lured on by various problems and unexpected digressions? The day is already ended and we have scarcely touched the subject proposed for discussion. Indeed we have deviated so far that I remember only with difficulty our early introduction and the little progress made in the way of hypotheses and principles for use in later demonstrations.

SAGREDO. Let us then adjourn for today in order that our minds may find refreshment in sleep and that we may return tomorrow, if so please you, and resume the discussion of the main question.

SALVIATI. I shall not fail to be here tomorrow at the same hour, hoping not only to render you service but also to enjoy your company.

END OF FIRST DAY

SECOND DAY

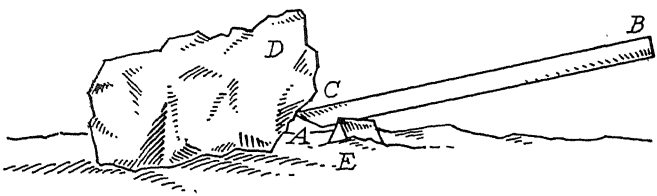
SAGREDO. While Simplicio and I were awaiting your arrival we were trying to recall that last consideration which you advanced as a principle and basis for the results you intended to obtain; this consideration dealt with the resistance which all solids offer to fracture and depended upon a certain cement which held the parts glued together so that they would yield and separate only under considerable pull. Later we tried to find the explanation of this coherence, seeking it mainly in the vacuum; this was the occasion of our many digressions which occupied the entire day and led us far afield from the original question which, as I have already stated, was the consideration of the resistance that solids offer to fracture.

SALVIATI. I remember it all very well. Resuming the thread of our discourse, whatever the nature of this resistance which solids offer to large tractive forces there can at least be no doubt of its existence; and though this resistance is very great in the case of a direct pull, it is found, as a rule, to be less in the case of bending forces. Thus, for example, a rod of steel or of glass will sustain a longitudinal pull of a thousand pounds while a weight of fifty pounds would be quite sufficient to break it if the rod were fastened at right angles into a vertical wall. It is this second type of resistance which we must consider, seeking to discover in what proportion it is found in prisms and cylinders of the same material, whether alike or unlike in shape, length, and thickness. In this discussion I shall take for granted the well-known mechanical principle which has been shown to govern the behavior of a bar, which we call a lever, namely, that the force bears to the resistance the inverse ratio of the distances which separate the fulcrum from the force and resistance respectively.

SIMPLICIO. This was demonstrated first of all by Aristotle, in his *Mechanics.*

SALVIATI. Yes, I am willing to concede him priority in point of time; but as regards rigor of demonstration the first place must be given to Archimedes. This principle established, I desire, before passing to any other subject, to call your attention to the fact that these forces, resistances, moments, figures, etc., may be considered either in the abstract, dissociated from matter, or in the concrete, associated with matter. Hence the properties which belong to figures that are merely geometrical and non-material must be modified when we fill these figures with matter and therefore give them weight. Take, for example, the lever BA which, resting upon the support E, is used to lift a heavy stone D. The principle just demonstrated makes it clear that a force applied at the extremity B will just suffice to equilibrate the resistance offered by the heavy body D provided this force bears to the force at D the same ratio as the distance AC bears to the distance CB; and this is true so long as we consider only the moments of the single force at B and of the resistance at D, treating the lever as an immaterial body devoid of weight. But if we take into account

the weight of the lever itself—an instrument which may be made either of wood or of iron—it is manifest that, when this weight has been added to the force at B, the ratio will be changed and must therefore be expressed in different terms. Hence before going further let us agree to distinguish between these two points of view; when we consider an instrument in the abstract, i. e., apart from the weight of its own material, we shall speak of



"taking it in an absolute sense"; but if we fill one of these simple and absolute figures with matter and thus give it weight, we shall refer to such a material figure as a "moment" or "compound force."

Let us now return to our original subject; then, if what has hitherto been said is clear, it will be easily understood that,

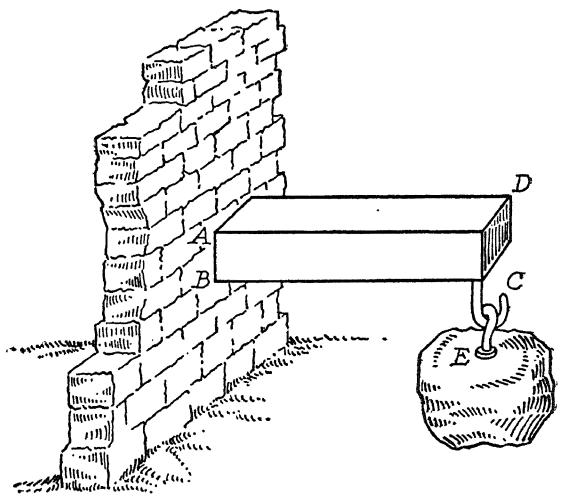
Proposition I

A prism or solid cylinder of glass, steel, wood or other breakable material which is capable of sustaining a very heavy weight when applied longitudinally is, as previously remarked, easily broken by the transverse application of a weight which may be much smaller in proportion as the length of the cylinder exceeds its thickness.

Let us imagine a solid prism ABCD fastened into a wall at the end AB, and supporting a weight E at the other end; understand also that the wall is vertical and that the prism or cylinder is fastened at right angles to the wall. It is clear that, if the cylinder breaks, fracture will occur at the point B where the edge of the mortise acts as a fulcrum for the lever BC, to which the force is applied; the thickness of the solid BA is the other arm of the lever along which is located the resistance. This resistance opposes the separation of the part BD, lying outside the wall, from that portion lying inside. From the preceding, it follows that the magnitude of the force applied at C bears to the magnitude of the resistance, found in the thickness of the prism, i. e., in the attachment of the base BA to its contiguous parts, the same ratio which the length CB bears to half the length BA; if now we define absolute resistance to fracture as that offered to a longitudinal pull (in which case the stretching force acts in the same direction as that through which the body is moved), then it follows that the absolute resistance of the prism BD is to the breaking load placed at the end of the lever BC in the same ratio as the length BC is to the half of AB in the case of a prism, or the semi-diameter in the case of a cylinder. This is our first proposition. Observe that in what has here been said

the weight of the solid BD itself has been left out of consideration, or rather, the prism has been assumed to be devoid of weight. But if the weight of the prism is to be taken account of in conjunction with the weight E, we must add to the weight E one half that of the prism BD: so that if, for example, the latter weighs two pounds and the weight E is ten pounds we must treat the weight E as if it were eleven pounds.

SIMPLICIO. Why not twelve?



SALVIATI. The weight E, my dear Simplicio, hanging at the extreme end C, acts upon the lever BC with its full moment of ten pounds: so also would the solid BD if suspended at the same point exert its full moment of two pounds; but, as you know, this solid is uniformly distributed throughout its entire length, BC, so that the parts which lie near the end B are less effective than those more remote.

Accordingly if we strike a balance between the two, the weight of the entire prism may be considered as concentrated at its center of gravity which lies midway of the lever BC. But a weight hung at the extremity C exerts a moment twice as great as it would if suspended from the middle: therefore if we consider the moments of both as located at the end C we must add to the weight E one-half that of the prism.

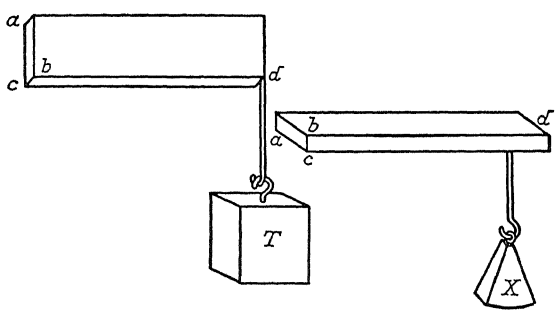
SIMPLICIO. I understand perfectly; and moreover, if I mistake not, the force of the two weights BD and E, thus disposed, would exert the same moment as would the entire weight BD together with twice the weight E suspended at the middle of the lever BC.

SALVIATI. Precisely so, and a fact worth remembering. Now we can readily understand

Proposition II

How and in what proportion a rod, or rather a prism, whose width is greater than its thickness offers more resistance to fracture when the force is applied in the direction of its breadth than in the direction of its thickness.

For the sake of clearness, take a ruler *ad* whose width is *ac* and whose thickness, *cb,* is much less than its width. The question now is why will the ruler, if stood on edge, as in the first figure, withstand a great weight T, while, when laid flat, as in the second figure, it will not support the weight X which is less than T. The answer is evident when we remember that in the one case the fulcrum is at the line *bc,* and in the other case at *ca,* while the distance at which the force is applied is the same in both cases, namely, the length *bd:* but in the first case the distance of the resistance from the fulcrum—half the line *ca*—is greater than in the other case where it is only half of *bc.* Therefore the weight T is greater than X in the same ratio as half the width *ca* is greater than half the thickness *bc,* since the former acts as a lever arm for *ca,* and the latter for *cb,* against



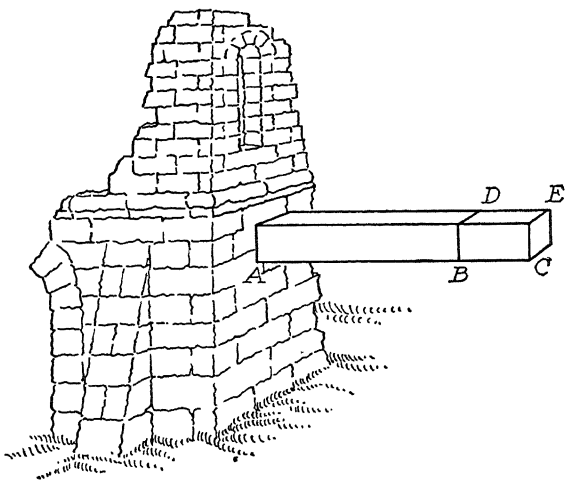
the same resistance, namely, the strength of all the fibres in the cross-section *ab.* We conclude, therefore, that any given ruler, or prism, whose width exceeds its thickness, will offer greater resistance to fracture when standing on edge than when lying flat, and this in the ratio of the width to the thickness.

Proposition III

Considering now the case of a prism or cylinder growing longer in a horizontal direction, we must find out in what ratio the moment of its own weight increases in comparison with its resistance to fracture. This moment I find increases in proportion to the square of the length. In order to prove this let AD be a prism or cylinder lying horizontal with its end A firmly fixed in a wall. Let the length of the prism be increased by the addition of the portion BE. It is clear that merely changing the length of the lever from AB to AC will, if we disregard its weight, increase the

moment of the force [at the end] tending to produce fracture at A in the ratio of CA to BA. But, besides this, the weight of the solid portion BE, added to the weight of the solid AB, increases the moment of the total weight in the ratio of the weight of the prism AE to that of the prism AB, which is the same as the ratio of the length AC to AB.

It follows, therefore, that, when the length and weight are simultaneously increased in any given proportion, the moment, which is the product of these two, is increased in a ratio which is the square of the preceding proportion. The conclusion is then that the bending moments due to the weight of prisms and cylinders which have the same thickness but



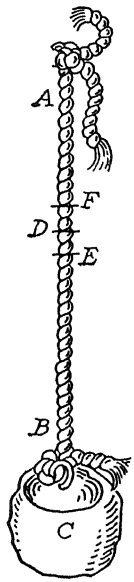
different lengths bear to each other a ratio which is the square of the ratio of their lengths, or, what is the same thing, the ratio of the squares of their lengths.

SIMPLICIO. Before proceeding further I should like to have one of my difficulties removed. Up to this point you have not taken into consideration a certain other kind of resistance which, it appears to me, diminishes as the solid grows longer, and this is quite as true in the case of bending as in pulling; it is precisely thus that in the case of a rope we observe that a very long one is less able to support a large weight than a short one. Whence, I believe, a short rod of wood or iron will support a greater weight than if it were long, provided the force be always applied longitudinally and not transversely, and provided also that we take into account the weight of the rope itself which increases with its length.

SALVIATI. I fear, Simplicio, if I correctly catch your meaning, that in this particular you are making the same mistake as many others; that is if you mean to say that a long rope, one of perhaps 40 cubits, cannot hold up so great a weight as a shorter length, say one or two cubits, of the same rope.

SIMPLICIO. That is what I meant, and as far as I see the proposition is highly probable.

SALVIATI. On the contrary, I consider it not merely improbable but false; and I think I can easily convince you of your error. Let AB represent the rope, fastened at the upper end A: at the lower end attach a weight C whose force is just sufficient to break the rope. Now, Simplicio, point out the exact place where you think the break ought to occur.



SIMPLICIO. Let us say D.

SALVIATI. And why at D?

SIMPLICIO. Because at this point the rope is not strong enough to support, say, 100 pounds, made up of the portion of the rope DB and the stone C.

SALVIATI. Accordingly whenever the rope is stretched with the weight of 100 pounds at D it will break there.

SIMPLICIO. I think so.

SALVIATI. But tell me, if instead of attaching the weight at the end of the rope, B, one fastens it at a point nearer D, say, at E: or if, instead of fixing the upper end of the rope at A, one fastens it at some point F, just above D, will not the rope, at the point D, be subject to the same pull of 100 pounds?

SIMPLICIO. It would, provided you include with the stone C the portion of rope EB.

SALVIATI. Let us therefore suppose that the rope is stretched at the point D with a weight of 100 pounds, then according to your own admission it will break; but FE is only a small portion of AB; how can you therefore maintain that the long rope is weaker than the short one? Give up then this erroneous view which you share with many very intelligent people, and let us proceed.

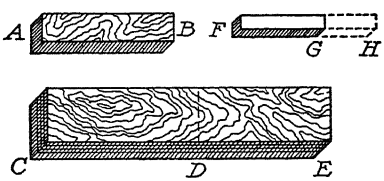
Proposition IV

Among heavy prisms and cylinders of similar figure, there is one and only one which under the stress of its own weight lies just on the limit between breaking and not breaking: so that every larger one is unable to carry the load of its own weight and breaks; while every smaller one is able to withstand some additional force tending to break it.

Let AB be a heavy prism, the longest possible that will just sustain its own weight, so that if it be lengthened the least bit it will break. Then, I say, this prism is unique among all similar prisms—infinite in number—in occupying that boundary line between breaking and not breaking; so that every larger one will break under its own weight, and every smaller

one will not break, but will be able to withstand some force in addition to its own weight.

Let the prism CE be similar to, but larger than, AB: then, I say, it will not remain intact but will break under its own weight. Lay off the portion CD, equal in length to AB. And since the resistance [bending strength] of CD is to that of AB as the cube of the thickness of CD is to the cube of the thickness of AB, that is, as the prism CE is to the similar prism AB, it follows that the weight of CE is the utmost load which a prism of the length CD can sustain; but the length of CE is greater; therefore the prism CE will break. Now take another prism FG which is smaller than AB. Let FH equal AB, then it can be shown in a similar manner that the resistance [bending strength] of FG is to that of AB as the prism FG is to the prism AB provided the distance AB that is FH is equal to the distance FG; but AB is greater than FG, and therefore the



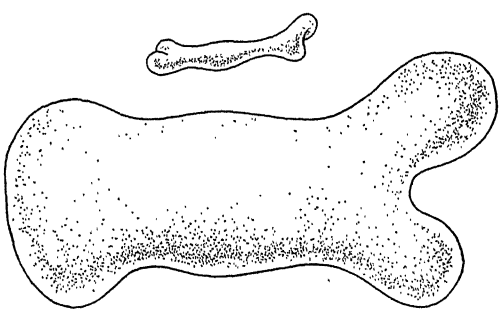
moment of the prism FG applied at G is not sufficient to break the prism FG.

SAGREDO. The demonstration is short and clear; while the proposition which, at first glance, appeared improbable is now seen to be both true and inevitable. In order therefore to bring this prism into that limiting condition which separates breaking from not breaking, it would be necessary to change the ratio between thickness and length either by increasing the thickness or by diminishing the length.

From what has already been demonstrated, you can plainly see the impossibility of increasing the size of structures to vast dimensions either in art or in nature; likewise the impossibility of building ships, palaces, or temples of enormous size in such a way that their oars, yards, beams, iron bolts, and, in short, all their other parts will hold together; nor can nature produce trees of extraordinary size because the branches would break down under their own weight; so also it would be impossible to build up the bony structures of men, horses, or other animals so as to hold together and perform their normal functions if these animals were to be increased enormously in height; for this increase in height can be accomplished only by employing a material which is harder and stronger than usual, or by enlarging the size of the bones, thus changing their shape until the form and appearance of the animals suggest a monstrosity. This is perhaps what our wise Poet [Ariosto] had in mind, when he says, in describing a huge giant:

> Impossible it is to reckon his height
> So beyond measure is his size.

To illustrate briefly, I have sketched a bone whose natural length has been increased three times and whose thickness has been multiplied until, for a correspondingly large animal, it would perform the same function which the small bone performs for its small animal. From the figures here shown you can see how out of proportion the enlarged bone appears. Clearly then if one wishes to maintain in a great giant the same proportion of limb as that found in an ordinary man he must either find a harder and stronger material for making the bones, or he must admit a diminution of strength in comparison with men of medium stature; for if his height be increased inordinately he will fall and be crushed under his own weight. Whereas, if the size of a body be diminished, the strength of that body is not diminished in the same proportion; indeed the smaller the body the greater its relative strength. Thus a small dog could prob-



ably carry on his back two or three dogs of his own size; but I believe that a horse could not carry even one of his own size.

SIMPLICIO. This may be so; but I am led to doubt it on account of the enormous size reached by certain fish, such as the whale which, I understand, is ten times as large as an elephant; yet they all support themselves.

SALVIATI. Your question, Simplicio, suggests another principle, one which had hitherto escaped my attention and which enables giants and other animals of vast size to support themselves and to move about as well as smaller animals do. This result may be secured either by increasing the strength of the bones and other parts intended to carry not only their weight but also the superincumbent load; or, keeping the proportions of the bony structure constant, the skeleton will hold together in the same manner or even more easily, provided one diminishes, in the proper proportion, the weight of the bony material, of the flesh, and of anything else which the skeleton has to carry. It is this second principle which is employed by nature in the structure of fish, making their bones and muscles not merely light but entirely devoid of weight.

SIMPLICIO. The trend of your argument, Salviati, is evident. Since fish live in water which on account of its density or, as others would say, heaviness diminishes the weight of bodies immersed in it, you mean to say that, for this reason, the bodies of fish will be devoid of weight and

will be supported without injury to their bones. But this is not all; for although the remainder of the body of the fish may be without weight, there can be no question but that their bones have weight. Take the case of a whale's rib, having the dimensions of a beam; who can deny its great weight or its tendency to go to the bottom when placed in water? One would, therefore, hardly expect these great masses to sustain themselves.

SALVIATI. A very shrewd objection! And now, in reply, tell me whether you have ever seen fish stand motionless at will under water, neither descending to the bottom nor rising to the top, without the exertion of force by swimming?

SIMPLICIO. This is a well-known phenomenon.

SALVIATI. The fact then that fish are able to remain motionless under water is a conclusive reason for thinking that the material of their bodies has the same specific gravity as that of water; accordingly, if in their make-up there are certain parts which are heavier than water there must be others which are lighter, for otherwise they would not produce equilibrium.

Hence, if the bones are heavier, it is necessary that the muscles or other constituents of the body should be lighter in order that their buoyancy may counterbalance the weight of the bones. In aquatic animals therefore circumstances are just reversed from what they are with land animals inasmuch as, in the latter, the bones sustain not only their own weight but also that of the flesh, while in the former it is the flesh which supports not only its own weight but also that of the bones. We must therefore cease to wonder why these enormously large animals inhabit the water rather than the land, that is to say, the air.

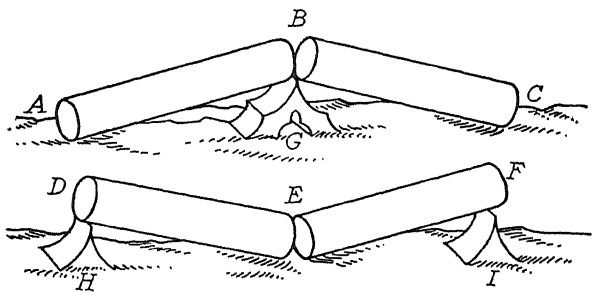
SIMPLICIO. I am convinced and I only wish to add that what we call land animals ought really to be called air animals, seeing that they live in the air, are surrounded by air, and breathe air.

SAGREDO. I have enjoyed Simplicio's discussion including both the question raised and its answer. Moreover I can easily understand that one of these giant fish, if pulled ashore, would not perhaps sustain itself for any great length of time, but would be crushed under its own mass as soon as the connections between the bones gave way.

SALVIATI. I am inclined to your opinion; and, indeed, I almost think that the same thing would happen in the case of a very big ship which floats on the sea without going to pieces under its load of merchandise and armament, but which on dry land and in air would probably fall apart. But let us proceed.

Hitherto we have considered the moments and resistances of prisms and solid cylinders fixed at one end with a weight applied at the other end; three cases were discussed, namely, that in which the applied force was the only one acting, that in which the weight of the prism itself is also taken into consideration, and that in which the weight of the prism alone is taken into consideration. Let us now consider these same prisms and cylinders when supported at both ends or at a single point placed somewhere between the ends. In the first place, I remark that a cylinder

carrying only its own weight and having the maximum length, beyond which it will break, will, when supported either in the middle or at both ends, have twice the length of one which is mortised into a wall and supported only at one end. This is very evident because, if we denote the cylinder by ABC and if we assume that one-half of it, AB, is the greatest possible length capable of supporting its own weight with one end fixed at B, then, for the same reason, if the cylinder is carried on the point C, the first half will be counterbalanced by the other half BC. So also in the case of the cylinder DEF, if its length be such that it will support only one-half this length when the end D is held fixed, or the other half when the end F is fixed, then it is evident that when supports, such as H and I, are placed under the ends D and F respectively the moment of any additional force or weight placed at E will produce fracture at this point.



SAGREDO. What shall we say, Simplicio? Must we not confess that geometry is the most powerful of all instruments for sharpening the wit and training the mind to think correctly? Was not Plato perfectly right when he wished that his pupils should be first of all well grounded in mathematics? As for myself, I quite understood the property of the lever and how, by increasing or diminishing its length, one can increase or diminish the moment of force and of resistance; and yet, in the solution of the present problem I was not slightly, but greatly, deceived.

SIMPLICIO. Indeed I begin to understand that while logic is an excellent guide in discourse, it does not, as regards stimulation to discovery, compare with the power of sharp distinction which belongs to geometry.

SAGREDO. Logic, it appears to me, teaches us how to test the conclusiveness of any argument or demonstration already discovered and completed; but I do not believe that it teaches us to discover correct arguments and demonstrations.

END OF SECOND DAY

THIRD DAY

CHANGE OF POSITION

MY PURPOSE is to set forth a very new science dealing with a very ancient subject. There is, in nature, perhaps nothing older than motion, concerning which the books written by philosophers are neither few nor small; nevertheless I have discovered by experiment some properties of it which are worth knowing and which have not hitherto been either observed or demonstrated. Some superficial observations have been made, as, for instance, that the free motion of a heavy falling body is continuously accelerated; but to just what extent this acceleration occurs has not yet been announced; for so far as I know, no one has yet pointed out that the distances traversed, during equal intervals of time, by a body falling from rest, stand to one another in the same ratio as the odd numbers beginning with unity.

It has been observed that missiles and projectiles describe a curved path of some sort; however no one has pointed out the fact that this path is a parabola. But this and other facts, not few in number or less worth knowing, I have succeeded in proving; and what I consider more important, there have been opened up to this vast and most excellent science, of which my work is merely the beginning, ways and means by which other minds more acute than mine will explore its remote corners.

NATURALLY ACCELERATED MOTION

The properties belonging to uniform motion have been discussed; but accelerated motion remains to be considered.

And first of all it seems desirable to find and explain a definition best fitting natural phenomena. For anyone may invent an arbitrary type of motion and discuss its properties; thus, for instance, some have imagined helices and conchoids as described by certain motions which are not met with in nature, and have very commendably established the properties which these curves possess in virtue of their definitions; but we have decided to consider the phenomena of bodies falling with an acceleration such as actually occurs in nature and to make this definition of accelerated motion exhibit the essential features of observed accelerated motions. And this, at last, after repeated efforts we trust we have succeeded in doing. In this belief we are confirmed mainly by the consideration that experimental results are seen to agree with and exactly correspond with those properties which have been, one after another, demonstrated by us. Finally, in the investigation of naturally accelerated motion we were led, by hand as it were, in following the habit and custom of nature herself, in all her various other processes, to employ only those means which are most common, simple and easy.

For I think no one believes that swimming or flying can be accomplished in a manner simpler or easier than that instinctively employed by fishes and birds.

When, therefore, I observe a stone initially at rest falling from an elevated position and continually acquiring new increments of speed, why should I not believe that such increases take place in a manner which is exceedingly simple and rather obvious to everybody? If now we examine the matter carefully we find no addition or increment more simple than that which repeats itself always in the same manner. This we readily understand when we consider the intimate relationship between time and motion; for just as uniformity of motion is defined by and conceived through equal times and equal spaces (thus we call a motion uniform when equal distances are traversed during equal time-intervals), so also we may, in a similar manner, through equal time-intervals, conceive additions of speed as taking place without complication; thus we may picture to our mind a motion as uniformly and continuously accelerated when, during any equal intervals of time whatever, equal increments of speed are given to it. Thus if any equal intervals of time whatever have elapsed, counting from the time at which the moving body left its position of rest and began to descend, the amount of speed acquired during the first two time-intervals will be double that acquired during the first time-interval alone; so the amount added during three of these time-intervals will be treble; and that in four, quadruple that of the first time-interval. To put the matter more clearly, if a body were to continue its motion with the same speed which it had acquired during the first time-interval and were to retain this same uniform speed, then its motion would be twice as slow as that which it would have if its velocity had been acquired during *two* time-intervals.

And thus, it seems, we shall not be far wrong if we put the increment of speed as proportional to the increment of time; hence the definition of motion which we are about to discuss may be stated as follows: A motion is said to be uniformly accelerated when, starting from rest, it acquires, during equal time-intervals, equal increments of speed.

SAGREDO. Although I can offer no rational objection to this or indeed to any other definition, devised by any author whomsoever, since all definitions are arbitrary, I may nevertheless without offense be allowed to doubt whether such a definition as the above, established in an abstract manner, corresponds to and describes that kind of accelerated motion which we meet in nature in the case of freely falling bodies. And since the Author apparently maintains that the motion described in his definition is that of freely falling bodies, I would like to clear my mind of certain difficulties in order that I may later apply myself more earnestly to the propositions and their demonstrations.

SALVIATI. It is well that you and Simplicio raise these difficulties. They are, I imagine, the same which occurred to me when I first saw this treatise, and which were removed either by discussion with the Author himself, or by turning the matter over in my own mind.

SAGREDO. When I think of a heavy body falling from rest, that is, starting with zero speed and gaining speed in proportion to the time from the beginning of the motion; such a motion as would, for instance, in eight beats of the pulse acquire eight degrees of speed; having at the end of the fourth beat acquired four degrees; at the end of the second, two; at the end of the first, one: and since time is divisible without limit, it follows from all these considerations that if the earlier speed of a body is less than its present speed in a constant ratio, then there is no degree of speed however small (or, one may say, no degree of slowness however great) with which we may not find this body travelling after starting from infinite slowness, i. e., from rest. So that if that speed which it had at the end of the fourth beat was such that, if kept uniform, the body would traverse two miles in an hour, and if keeping the speed which it had at the end of the second beat, it would traverse one mile an hour, we must infer that, as the instant of starting is more and more nearly approached, the body moves so slowly that, if it kept on moving at this rate, it would not traverse a mile in an hour, or in a day, or in a year or in a thousand years; indeed, it would not traverse a span in an even greater time; a phenomenon which baffles the imagination, while our senses show us that a heavy falling body suddenly acquires great speed.

SALVIATI. This is one of the difficulties which I also at the beginning experienced, but which I shortly afterwards removed; and the removal was effected by the very experiment which creates the difficulty for you. You say the experiment appears to show that immediately after a heavy body starts from rest it acquires a very considerable speed: and I say that the same experiment makes clear the fact that the initial motions of a falling body, no matter how heavy, are very slow and gentle. Place a heavy body upon a yielding material, and leave it there without any pressure except that owing to its own weight; it is clear that if one lifts this body a cubit or two and allows it to fall upon the same material, it will, with this impulse, exert a new and greater pressure than that caused by its mere weight; and this effect is brought about by the [weight of the] falling body together with the velocity acquired during the fall, an effect which will be greater and greater according to the height of the fall, that is, according as the velocity of the falling body becomes greater. From the quality and intensity of the blow we are thus enabled to accurately estimate the speed of a falling body. But tell me, gentlemen, is it not true that if a block be allowed to fall upon a stake from a height of four cubits and drives it into the earth, say, four fingerbreadths, that coming from a height of two cubits it will drive the stake a much less distance, and from the height of one cubit a still less distance; and finally if the block be lifted only one fingerbreadth how much more will it accomplish than if merely laid on top of the stake without percussion? Certainly very little. If it be lifted only the thickness of a leaf, the effect will be altogether imperceptible. And since the effect of the blow depends upon the velocity of this striking body, can anyone doubt the motion is very slow and the speed more than small whenever the effect [of the blow] is imperceptible?

See now the power of truth; the same experiment which at first glance seemed to show one thing, when more carefully examined, assures us of the contrary.

But without depending upon the above experiment, which is doubtless very conclusive, it seems to me that it ought not to be difficult to establish such a fact by reasoning alone. Imagine a heavy stone held in the air at rest; the support is removed and the stone set free; then since it is heavier than the air it begins to fall, and not with uniform motion but slowly at the beginning and with a continuously accelerated motion. Now since velocity can be increased and diminished without limit, what reason is there to believe that such a moving body starting with infinite slowness, that is, from rest, immediately acquires a speed of ten degrees rather than one of four, or of two, or of one, or of a half, or of a hundredth; or, indeed, of any of the infinite number of small values [of speed]? Pray listen. I hardly think you will refuse to grant that the gain of speed of the stone falling from rest follows the same sequence as the diminution and loss of this same speed when, by some impelling force, the stone is thrown to its former elevation: but even if you do not grant this, I do not see how you can doubt that the ascending stone, diminishing in speed, must before coming to rest pass through every possible degree of slowness.

Simplicio. But if the number of degrees of greater and greater slowness is limitless, they will never be all exhausted, therefore such an ascending heavy body will never reach rest, but will continue to move without limit always at a slower rate; but this is not the observed fact.

Salviati. This would happen, Simplicio, if the moving body were to maintain its speed for any length of time at each degree of velocity; but it merely passes each point without delaying more than an instant: and since each time-interval however small may be divided into an infinite number of instants, these will always be sufficient [in number] to correspond to the infinite degrees of diminished velocity.

That such a heavy rising body does not remain for any length of time at any given degree of velocity is evident from the following: because if, some time-interval having been assigned, the body moves with the same speed in the last as in the first instant of that time-interval, it could from this second degree of elevation be in like manner raised through an equal height, just as it was transferred from the first elevation to the second, and by the same reasoning would pass from the second to the third and would finally continue in uniform motion forever.

Salviati. The present does not seem to be the proper time to investigate the cause of the acceleration of natural motion concerning which various opinions have been expressed by various philosophers, some explaining it by attraction to the center, others to repulsion between the very small parts of the body, while still others attribute it to a certain stress in the surrounding medium which closes in behind the falling body and drives it from one of its positions to another. Now, all these fantasies, and others too, ought to be examined; but it is not really worth while. At present it is the purpose of our Author merely to investigate and to

demonstrate some of the properties of accelerated motion (whatever the cause of this acceleration may be)—meaning thereby a motion, such that the momentum of its velocity goes on increasing after departure from rest, in simple proportionality to the time, which is the same as saying that in equal time-intervals the body receives equal increments of velocity; and if we find the properties [of accelerated motion] which will be demonstrated later are realized in freely falling and accelerated bodies, we may conclude that the assumed definition includes such a motion of falling bodies and that their speed goes on increasing as the time and the duration of the motion.

SAGREDO. So far as I see at present, the definition might have been put a little more clearly perhaps without changing the fundamental idea, namely, uniformly accelerated motion is such that its speed increases in proportion to the space traversed; so that, for example, the speed acquired by a body in falling four cubits would be double that acquired in falling two cubits and this latter speed would be double that acquired in the first cubit. Because there is no doubt but that a heavy body falling from the height of six cubits has, and strikes with, a momentum double that it had at the end of three cubits, triple that which it would have if it had fallen from two, and sextuple that which it would have had at the end of one.

SALVIATI. It is very comforting to me to have had such a companion in error; and moreover let me tell you that your proposition seems so highly probable that our Author himself admitted, when I advanced this opinion to him, that he had for some time shared the same fallacy. But what most surprised me was to see two propositions so inherently probable that they commanded the assent of everyone to whom they were presented, proven in a few simple words to be not only false, but impossible.

SIMPLICIO. I am one of those who accept the proposition, and believe that a falling body acquires force in its descent, its velocity increasing in proportion to the space, and that the momentum of the falling body is doubled when it falls from a doubled height; these propositions, it appears to me, ought to be conceded without hesitation or controversy.

SALVIATI. And yet they are as false and impossible as that motion should be completed instantaneously; and here is a very clear demonstration of it. If the velocities are in proportion to the spaces traversed, or to be traversed, then these spaces are traversed in equal intervals of time; if, therefore, the velocity with which the falling body traverses a space of eight feet were double that with which it covered the first four feet (just as the one distance is double the other) then the time-intervals required for these passages would be equal. But for one and the same body to fall eight feet and four feet in the same time is possible only in the case of instantaneous [discontinuous] motion; but observation shows us that the motion of a falling body occupies time, and less of it in covering a distance of four feet than of eight feet; therefore it is not true that its velocity increases in proportion to the space.

The falsity of the other proposition may be shown with equal clear-

ness. For if we consider a single striking body the difference of momentum in its blows can depend only upon difference of velocity; for if the striking body falling from a double height were to deliver a blow of double momentum, it would be necessary for this body to strike with a doubled velocity; but with this doubled speed it would traverse a doubled space in the same time-interval; observation however shows that the time required for fall from the greater height is longer.

SAGREDO. You present these recondite matters with too much evidence and ease; this great facility makes them less appreciated than they would be had they been presented in a more abstruse manner. For, in my opinion, people esteem more lightly that knowledge which they acquire with so little labor than that acquired through long and obscure discussion.

SALVIATI. If those who demonstrate with brevity and clearness the fallacy of many popular beliefs were treated with contempt instead of gratitude the injury would be quite bearable; but on the other hand it is very unpleasant and annoying to see men, who claim to be peers of anyone in a certain field of study, take for granted certain conclusions which later are quickly and easily shown by another to be false. I do not describe such a feeling as one of envy, which usually degenerates into hatred and anger against those who discover such fallacies; I would call it a strong desire to maintain old errors, rather than accept newly discovered truths. This desire at times induces them to unite against these truths, although at heart believing in them, merely for the purpose of lowering the esteem in which certain others are held by the unthinking crowd. Indeed, I have heard from our Academician many such fallacies held as true but easily refutable; some of these I have in mind.

SAGREDO. You must not withhold them from us, but, at the proper time, tell us about them even though an extra session be necessary. But now, continuing the thread of our talk, it would seem that up to the present we have established the definition of uniformly accelerated motion which is expressed as follows:

> A motion is said to be equally or uniformly accelerated when, starting from rest, its momentum receives equal increments in equal times.

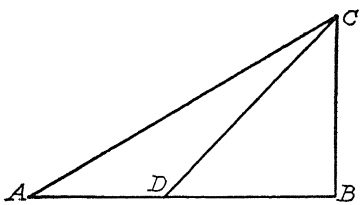
SALVIATI. This definition established, the Author makes a single assumption, namely,

> The speeds acquired by one and the same body moving down planes of different inclinations are equal when the heights of these planes are equal.

By the height of an inclined plane we mean the perpendicular let fall from the upper end of the plane upon the horizontal line drawn through the lower end of the same plane. Thus, to illustrate, let the line AB be horizontal, and let the planes CA and CD be inclined to it; then the Author calls the perpendicular CB the "height" of the planes CA and CD; he supposes that the speeds acquired by one and the same body, descending along the planes CA and CD to the terminal points A and D, are equal since the heights of these planes are the same, CB; and also it

must be understood that this speed is that which would be acquired by the same body falling from C to B.

SAGREDO. Your assumption appears to me so reasonable that it ought to be conceded without question, provided of course there are no chance or outside resistances, and that the planes are hard and smooth, and that the figure of the moving body is perfectly round, so that neither plane nor moving body is rough. All resistance and opposition having been removed, my reason tells me at once that a heavy and perfectly round ball descending along the lines CA, CD, CB would reach the terminal points A, D, B, with equal momenta.

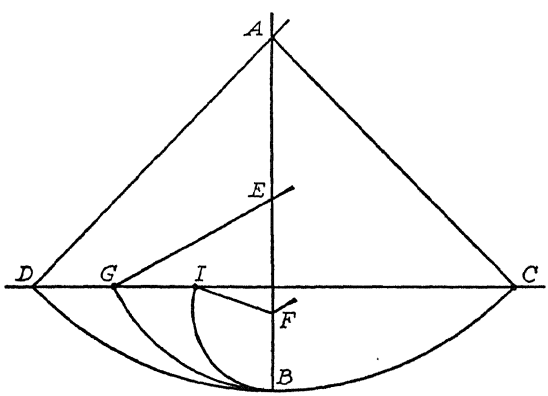


SALVIATI. Your words are very plausible; but I hope by experiment to increase the probability to an extent which shall be little short of a rigid demonstration.

Imagine this page to represent a vertical wall, with a nail driven into it; and from the nail let there be suspended a lead bullet of one or two ounces by means of a fine vertical thread, AB, say from four to six feet long. On this wall draw a horizontal line DC, at right angles to the vertical thread AB, which hangs about two fingerbreadths in front of the wall. Now bring the thread AB with the attached ball into the position AC and set it free; first it will be observed to descend along the arc CBD, to pass the point B, and to travel along the arc BD, till it almost reaches the horizontal CD, a slight shortage being caused by the resistance of the air and the string; from this we may rightly infer that the ball in its descent through the arc CB acquired a momentum on reaching B, which was just sufficient to carry it through a similar arc BD to the same height. Having repeated this experiment many times, let us now drive a nail into the wall close to the perpendicular AB, say at E or F, so that it projects out some five or six fingerbreadths in order that the thread, again carrying the bullet through the arc CB, may strike upon the nail E when the bullet reaches B, and thus compel it to traverse the arc BG, described about E as center. From this we can see what can be done by the same momentum which previously starting at the same point B carried the same body through the arc BD to the horizontal CD. Now, gentlemen, you will observe with pleasure that the ball swings to the point G in the horizontal, and you would see the same thing happen if the obstacle were placed at some lower point, say at F, about which the ball would describe the arc BI, the rise of the ball always terminating exactly on the line CD. But when the nail is placed so low that the remainder of the thread below it

will not reach to the height CD (which would happen if the nail were placed nearer B than to the intersection of AB with the horizontal CD) then the thread leaps over the nail and twists itself about it.

This experiment leaves no room for doubt as to the truth of our supposition; for since the two arcs CB and DB are equal and similarly placed, the momentum acquired by the fall through the arc CB is the same as that gained by fall through the arc DB; but the momentum acquired at B, owing to fall through CB, is able to lift the same body through the arc BD; therefore, the momentum acquired in the fall BD is equal to that which lifts the same body through the same arc from B to D; so, in general, every momentum acquired by fall through an arc is equal to that which can lift the same body through the same arc. But all these momenta which cause a rise through the arcs BD, BG, and BI are



equal, since they are produced by the same momentum, gained by fall through CB, as experiment shows. Therefore all the momenta gained by fall through the arcs DB, GB, IB are equal.

SAGREDO. The argument seems to me so conclusive and the experiment so well adapted to establish the hypothesis that we may, indeed, consider it as demonstrated.

SALVIATI. I do not wish, Sagredo, that we trouble ourselves too much about this matter, since we are going to apply this principle mainly in motions which occur on plane surfaces, and not upon curved, along which acceleration varies in a manner greatly different from that which we have assumed for planes.

So that, although the above experiment shows us that the descent of the moving body through the arc CB confers upon it momentum just sufficient to carry it to the same height through any of the arcs BD, BG, BI, we are not able, by similar means, to show that the event would be identical in the case of a perfectly round ball descending along planes whose inclinations are respectively the same as the chords of these arcs. It seems likely, on the other hand, that, since these planes form angles at

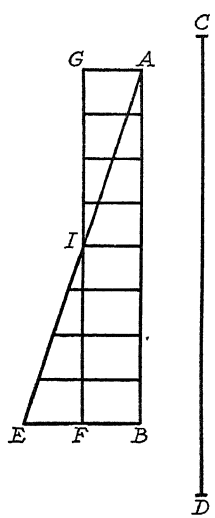
the point B, they will present an obstacle to the ball which has descended along the chord CB and starts to rise along the chord BD, BG, BI.

In striking these planes some of its momentum will be lost and it will not be able to rise to the height of the line CD; but this obstacle, which interferes with the experiment, once removed, it is clear that the momentum (which gains in strength with descent) will be able to carry the body to the same height. Let us then, for the present, take this as a postulate, the absolute truth of which will be established when we find that the inferences from it correspond to and agree perfectly with experiment. The Author having assumed this single principle passes next to the propositions which he clearly demonstrates; the first of these is as follows:

Theorem I, Proposition I

> The time in which any space is traversed by a body starting from rest and uniformly accelerated is equal to the time in which that same space would be traversed by the same body moving at a uniform speed whose value is the mean of the highest speed and the speed just before acceleration began.

Let us represent by the line AB the time in which the space CD is traversed by a body which starts from rest at C and is uniformly accelerated; let the final and highest value of the speed gained during the interval AB be represented by the line EB drawn at right angles to AB; draw the line AE, then all lines drawn from equidistant points on AB and parallel to BE will represent the increasing values of the speed, beginning with the instant A. Let the point F bisect the line EB; draw FG parallel to BA, and GA parallel to FB, thus forming a parallelogram AGFB which will be equal in area to the triangle AEB, since the side GF bisects the side AE at the point I; for if the parallel lines in the triangle AEB are extended to GI, then the sum of all the parallels contained in the quadrilateral is equal to the sum of those contained in the triangle AEB; for those in the triangle IEF are equal to those contained in the triangle GIA, while those included in the trapezium AIFB are common. Since each and every instant of time in the time-interval AB has its corresponding point on the line AB, from which points parallels drawn in and limited by the triangle AEB represent the increasing values of the growing velocity, and since parallels contained within the rectangle represent the values of a speed which is not increasing, but constant, it appears, in like manner, that the momenta assumed by the moving body may also be represented, in the case of the accelerated



motion, by the increasing parallels of the triangle AEB, and, in the case of the uniform motion, by the parallels of the rectangle GB. For, what the momenta may lack in the first part of the accelerated motion (the deficiency of the momenta being represented by the parallels of the triangle AGI) is made up by the momenta represented by the parallels of the triangle IEF.

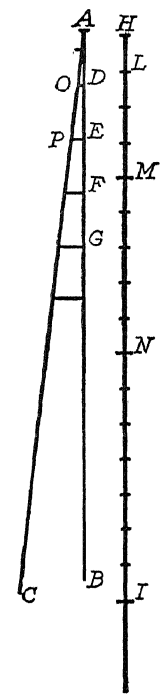
Hence it is clear that equal spaces will be traversed in equal times by two bodies, one of which, starting from rest, moves with a uniform acceleration, while the momentum of the other, moving with uniform speed, is one-half its maximum momentum under accelerated motion.

Q.E.D.

Theorem II, Proposition II

The spaces described by a body falling from rest with a uniformly accelerated motion are to each other as the squares of the time-intervals employed in traversing these distances.

Let the time beginning with any instant A be represented by the straight line AB in which are taken any two time-intervals AD and AE. Let HI represent the distance through which the body, starting from rest at H, falls with uniform acceleration. If HL represents the space traversed during the time-interval AD, and HM that covered during the interval AE, then the space MH stands to the space LH in a ratio which is the square of the ratio of the time AE to the time AD; or we may say simply that the distances HM and HL are related as the squares of AE and AD.



Draw the line AC making any angle whatever with the line AB; and from the points D and E, draw the parallel lines DO and EP; of these two lines, DO represents the greatest velocity attained during the interval AD, while EP represents the maximum velocity acquired during the interval AE. But it has just been proved that so far as distances traversed are concerned it is precisely the same whether a body falls from rest with a uniform acceleration or whether it falls during an equal time-interval with a constant speed which is one-half the maximum speed attained during the accelerated motion. It follows therefore that the distances HM and HL are the same as would be traversed, during the time-intervals AE and AD, by uniform velocities equal to one-half those represented by DO and EP respectively. If, therefore, one can show that the distances HM and HL are in the same ratio as the

squares of the time-intervals AE and AD, our proposition will be proven.

It has been shown that the spaces traversed by two particles in uniform motion bear to one another a ratio which is equal to the product of the ratio of the velocities by the ratio of the times. But in this case the ratio of the velocities is the same as the ratio of the time-intervals (for the ratio of AE to AD is the same as that of ½EP to ½DO or of EP to DO). Hence the ratio of the spaces traversed is the same as the squared ratio of the time-intervals. Q.E.D.

Evidently then the ratio of the distances is the square of the ratio of the final velocities, that is, of the lines EP and DO, since these are to each other as AE to AD.

Corollary

Hence it is clear that if we take any equal intervals of time whatever, counting from the beginning of the motion, such as AD, DE, EF, FG, in which the spaces HL, LM, MN, NI are traversed, these spaces will bear to one another the same ratio as the series of odd numbers, 1, 3, 5, 7; for this is the ratio of the differences of the squares of the lines [which represent time], differences which exceed one another by equal amounts, this excess being equal to the smallest line [viz. the one representing a single time-interval]: or we may say [that this is the ratio] of the differences of the squares of the natural numbers beginning with unity.

While, therefore, during equal intervals of time the velocities increase as the natural numbers, the increments in the distances traversed during these equal time-intervals are to one another as the odd numbers beginning with unity.

SIMPLICIO. I am convinced that matters are as described, once having accepted the definition of uniformly accelerated motion. But as to whether this acceleration is that which one meets in nature in the case of falling bodies, I am still doubtful; and it seems to me, not only for my own sake but also for all those who think as I do, that this would be the proper moment to introduce one of those experiments—and there are many of them, I understand—which illustrate in several ways the conclusions reached.

SALVIATI. The request which you, as a man of science, make, is a very reasonable one; for this is the custom—and properly so—in those sciences where mathematical demonstrations are applied to natural phenomena, as is seen in the case of perspective, astronomy, mechanics, music, and others where the principles, once established by well-chosen experiments, become the foundations of the entire superstructure. I hope therefore it will not appear to be a waste of time if we discuss at considerable length this first and most fundamental question upon which hinge numerous consequences of which we have in this book only a small number, placed there by the Author, who has done so much to open a pathway hitherto closed to minds of speculative turn. So far as experiments go they have

not been neglected by the Author; and often, in his company, I have attempted in the following manner to assure myself that the acceleration actually experienced by falling bodies is that above described.

A piece of wooden moulding or scantling, about 12 cubits long, half a cubit wide, and three fingerbreadths thick, was taken; on its edge was cut a channel a little more than one finger in breadth; having made this groove very straight, smooth, and polished, and having lined it with parchment, also as smooth and polished as possible, we rolled along it a hard, smooth, and very round bronze ball. Having placed this board in a sloping position, by lifting one end some one or two cubits above the other, we rolled the ball, as I was just saying, along the channel, noting, in a manner presently to be described, the time required to make the descent. We repeated this experiment more than once in order to measure the time with an accuracy such that the deviation between two observations never exceeded one-tenth of a pulse beat. Having performed this operation and having assured ourselves of its reliability, we now rolled the ball only one-quarter the length of the channel; and having measured the time of its descent, we found it precisely one-half of the former. Next we tried other distances, comparing the time for the whole length with that for the half, or with that for two-thirds, or three-fourths, or indeed for any fraction; in such experiments, repeated a full hundred times, we always found that the spaces traversed were to each other as the squares of the times, and this was true for all inclinations of the plane, i. e., of the channel, along which we rolled the ball. We also observed that the times of descent, for various inclinations of the plane, bore to one another precisely that ratio which, as we shall see later, the Author had predicted and demonstrated for them.

For the measurement of time, we employed a large vessel of water placed in an elevated position; to the bottom of this vessel was soldered a pipe of small diameter giving a thin jet of water, which we collected in a small glass during the time of each descent, whether for the whole length of the channel or for a part of its length; the water thus collected was weighed, after each descent, on a very accurate balance; the differences and ratios of these weights gave us the differences and ratios of the times, and this with such accuracy that although the operation was repeated many, many times, there was no appreciable discrepancy in the results.

SIMPLICIO. I would like to have been present at these experiments; but feeling confidence in the care with which you performed them, and in the fidelity with which you relate them, I am satisfied and accept them as true and valid.

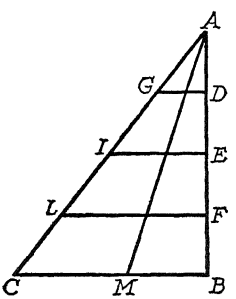
SALVIATI. Then we can proceed without discussion.

Theorem III, Proposition III

If one and the same body, starting from rest, falls along an inclined plane and also along a vertical, each having the same height, the

times of descent will be to each other as the lengths of the inclined plane and the vertical.

Let AC be the inclined plane and AB the perpendicular, each having the same vertical height above the horizontal, namely, BA; then I say, the time of descent of one and the same body along the plane AC bears a ratio to the time of fall along the perpendicular AB, which is the same as the ratio of the length AC to the length AB. Let DG, EI and LF be any lines parallel to the horizontal CB; then it follows from what has preceded that a body starting from A will acquire the same speed at the point G as at D, since in each case the vertical fall is the same; in like manner the speeds at I and E will be the same; so also those at L and F. And in general the speeds at the two extremities of any parallel drawn from any point on AB to the corresponding point on AC will be equal.



Thus the two distances AC and AB are traversed at the same speed. But it has already been proved that if two distances are traversed by a body moving with equal speeds, then the ratio of the times of descent will be the ratio of the distances themselves; therefore, the time of descent along AC is to that along AB as the length of the plane AC is to the vertical distance AB. Q.E.D.

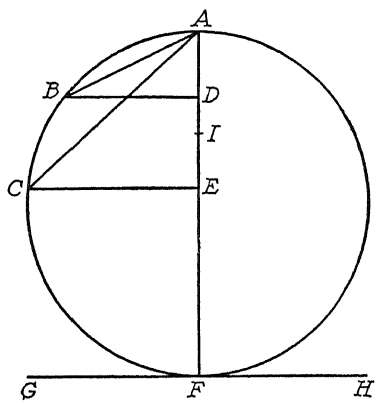
SAGREDO. It seems to me that the above could have been proved clearly and briefly on the basis of a proposition already demonstrated, namely, that the distance traversed in the case of accelerated motion along AC or AB is the same as that covered by a uniform speed whose value is one-half the maximum speed, CB; the two distances AC and AB having been traversed at the same uniform speed it is evident, from Proposition I, that the times of descent will be to each other as the distances.

Theorem IV, Proposition IV

If from the highest or lowest point in a vertical circle there be drawn any inclined planes meeting the circumference the times of descent along these chords are each equal to the other.

On the horizontal line GH construct a vertical circle. From its lowest point—the point of tangency with the horizontal—draw the diameter FA

and from the highest point, A, draw inclined planes to B and C, any points whatever on the circumference; then the times of descent along these are equal. Draw BD and CE perpendicular to the diameter; make AI a mean proportional between the heights of the planes, AE and AD; and since the rectangles FA.AE and FA.AD are respectively equal to the squares of AC and AB, while the rectangle FA.AE is to the rectangle FA.AD as AE is to AD, it follows that the square of AC is to the square of AB as the length AE is to the length AD. But since the length AE is to AD as the square of AI is to the square of AD, it follows that the squares on the lines AC and AB are to each other as the squares on the lines AI and AD, and hence also the length AC is to the length AB as AI



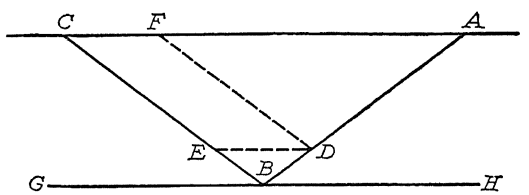
is to AD. But it has previously been demonstrated that the ratio of the time of descent along AC to that along AB is equal to the product of the two ratios AC to AB and AD to AI; but this last ratio is the same as that of AB to AC. Therefore the ratio of the time of descent along AC to that along AB is the product of the two ratios, AC to AB and AB to AC. The ratio of these times is therefore unity. Hence follows our proposition.

By use of the principles of mechanics one may obtain the same result.

Scholium

We may remark that any velocity once imparted to a moving body will be rigidly maintained as long as the external causes of acceleration or retardation are removed, a condition which is found only on horizontal planes; for in the case of planes which slope downwards there is already present a cause of acceleration, while on planes sloping upward there is retardation; from this it follows that motion along a horizontal plane is perpetual; for, if the velocity be uniform, it cannot be diminished or slackened, much less destroyed. Further, although any velocity which a body may have acquired through natural fall is permanently maintained so far as

its own nature is concerned, yet it must be remembered that if, after descent along a plane inclined downwards, the body is deflected to a plane inclined upwards, there is already existing in this latter plane a cause of retardation; for in any such plane this same body is subject to a natural acceleration downwards. Accordingly we have here the superposition of two different states, namely, the velocity acquired during the preceding fall which if acting alone would carry the body at a uniform rate to infinity, and the velocity which results from a natural acceleration downwards common to all bodies. It seems altogether reasonable, therefore, if we wish to trace the future history of a body which has descended along some inclined plane and has been deflected along some plane inclined upwards, for us to assume that the maximum speed acquired during descent is permanently maintained during the ascent. In the ascent, however, there supervenes a natural inclination downwards, namely, a motion which, starting from rest, is accelerated at the usual rate. If perhaps this discussion is a little obscure, the following figure will help to make it clearer.

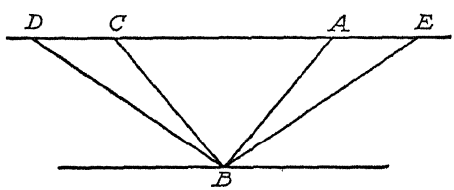


Let us suppose that the descent has been made along the downward sloping plane AB, from which the body is deflected so as to continue its motion along the upward sloping plane BC; and first let these planes be of equal length and placed so as to make equal angles with the horizontal line GH. Now it is well known that a body, starting from rest at A, and descending along AB, acquires a speed which is proportional to the time, which is a maximum at B, and which is maintained by the body so long as all causes of fresh acceleration or retardation are removed; the acceleration to which I refer is that to which the body would be subject if its motion were continued along the plane AB extended, while the retardation is that which the body would encounter if its motion were deflected along the plane BC inclined upwards; but, upon the horizontal plane GH, the body would maintain a uniform velocity equal to that which it had acquired at B after fall from A; moreover this velocity is such that, during an interval of time equal to the time of descent through AB, the body will traverse a horizontal distance equal to twice AB. Now let us imagine this same body to move with the same uniform speed along the plane BC so that here also during a time-interval equal to that of descent along AB, it will traverse along BC extended a distance twice AB; but let us suppose that, at the very instant the body begins its ascent it is subjected, by its very nature, to the same influences which surrounded it during its descent from A along AB, namely, it descends from rest under the same acceler-

ation as that which was effective in AB, and it traverses, during an equal interval of time, the same distance along this second plane as it did along AB; it is clear that, by thus superposing upon the body a uniform motion of ascent and an accelerated motion of descent, it will be carried along the plane BC as far as the point C where these two velocities become equal.

If now we assume any two points D and E, equally distant from the vertex B, we may then infer that the descent along BD takes place in the same time as the ascent along BE. Draw DF parallel to BC; we know that, after descent along AD, the body will ascend along DF; or, if, on reaching D, the body is carried along the horizontal DE, it will reach E with the same momentum with which it left D; hence from E the body will ascend as far as C, proving that the velocity at E is the same as that at D.

From this we may logically infer that a body which descends along any inclined plane and continues its motion along a plane inclined upwards will, on account of the momentum acquired, ascend to an equal



height above the horizontal; so that if the descent is along AB the body will be carried up the plane BC as far as the horizontal line ACD: and this is true whether the inclinations of the planes are the same or different, as in the case of the planes AB and BD. But by a previous postulate the speeds acquired by fall along variously inclined planes having the same vertical height are the same. If therefore the planes EB and BD have the same slope, the descent along EB will be able to drive the body along BD as far as D; and since this propulsion comes from the speed acquired on reaching the point B, it follows that this speed at B is the same whether the body has made its descent along AB or EB. Evidently then the body will be carried up BD whether the descent has been made along AB or along EB. The time of ascent along BD is however greater than that along BC, just as the descent along EB occupies more time than that along AB; moreover it has been demonstrated that the ratio between the lengths of these times is the same as that between the lengths of the planes.

Conclusion

SAGREDO. Indeed, I think we may concede to our Academician, without flattery, his claim that in the principle laid down in this treatise he has established a new science dealing with a very old subject. Observing

with what ease and clearness he deduces from a single principle the proofs of so many theorems, I wonder not a little how such a question escaped the attention of Archimedes, Apollonius, Euclid and so many other mathematicians and illustrious philosophers, especially since so many ponderous tomes have been devoted to the subject of motion.

SALVIATI. There is a fragment of Euclid which treats of motion, but in it there is no indication that he ever began to investigate the property of acceleration and the manner in which it varies with slope. So that we may say the door is now opened, for the first time, to a new method fraught with numerous and wonderful results which in future years will command the attention of other minds.

SAGREDO. I really believe that just as, for instance, the few properties of the circle proven by Euclid in the Third Book of his *Elements* lead to many others more recondite, so the principles which are set forth in this little treatise will, when taken up by speculative minds, lead to many another more remarkable result; and it is to be believed that it will be so on account of the nobility of the subject, which is superior to any other in nature.

During this long and laborious day, I have enjoyed these simple theorems more than their proofs, many of which, for their complete comprehension, would require more than an hour each; this study, if you will be good enough to leave the book in my hands, is one which I mean to take up at my leisure after we have read the remaining portion which deals with the motion of projectiles; and this if agreeable to you we shall take up tomorrow.

SALVIATI. I shall not fail to be with you.

END OF THIRD DAY

FOURTH DAY

SALVIATI. Once more, Simplicio is here on time; so let us without delay take up the question of motion. The text of our Author is as follows:

THE MOTION OF PROJECTILES

In the preceding pages we have discussed the properties of motion naturally accelerated. I now propose to set forth those properties which belong to a body whose motion is compounded of two other motions, namely, one uniform and one naturally accelerated; these properties, well worth knowing, I propose to demonstrate in a rigid manner. This is the kind of motion seen in a moving projectile; its origin I conceive to be as follows:

Imagine any particle projected along a horizontal plane without fric-

tion; then we know, from what has been more fully explained in the preceding pages, that this particle will move along this same plane with a motion which is uniform and perpetual, provided the plane has no limits. But if the plane is limited and elevated, then the moving particle, which we imagine to be a heavy one, will on passing over the edge of the plane acquire, in addition to its previous uniform and perpetual motion, a downward propensity due to its own weight; so that the resulting motion which I call projection is compounded of one which is uniform and horizontal and of another which is vertical and naturally accelerated. We now proceed to demonstrate some of its properties, the first of which is as follows:

Theorem I, Proposition I

A projectile which is carried by a uniform horizontal motion compounded with a naturally accelerated vertical motion describes a path which is a semi-parabola.

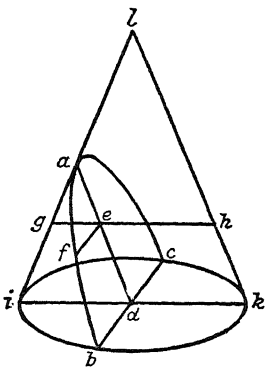
SAGREDO. Here, Salviati, it will be necessary to stop a little while for my sake and, I believe, also for the benefit of Simplicio; for it so happens that I have not gone very far in my study of Apollonius and am merely aware of the fact that he treats of the parabola and other conic sections, without an understanding of which I hardly think one will be able to follow the proof of other propositions depending upon them. Since even in this first beautiful theorem the Author finds it necessary to prove that the path of a projectile is a parabola, and since, as I imagine, we shall have to deal with only this kind of curves, it will be absolutely necessary to have a thorough acquaintance, if not with all the properties which Apollonius has demonstrated for these figures, at least with those which are needed for the present treatment.

SIMPLICIO. Now even though Sagredo is, as I believe, well equipped for all his needs, I do not understand even the elementary terms; for although our philosophers have treated the motion of projectiles, I do not recall their having described the path of a projectile except to state in a general way that it is always a curved line, unless the projection be vertically upwards. But if the little Euclid which I have learned since our previous discussion does not enable me to understand the demonstrations which are to follow, then I shall be obliged to accept the theorems on faith without fully comprehending them.

SALVIATI. On the contrary, I desire that you should understand them from the Author himself, who, when he allowed me to see this work of his, was good enough to prove for me two of the principal properties of the parabola because I did not happen to have at hand the books of Apollonius. These properties, which are the only ones we shall need in the present discussion, he proved in such a way that no prerequisite knowledge was required. These theorems are, indeed, given by Apollonius, but after many preceding ones, to follow which would take a long while. I

wish to shorten our task by deriving the first property purely and simply from the mode of generation of the parabola.

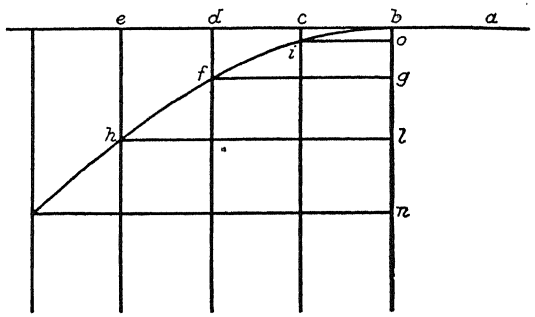
Beginning now with the first, imagine a right cone, erected upon the circular base *ibkc* with apex at *l*. The section of this cone made by a plane drawn parallel to the side *lk* is the curve which is called a *parabola*. The base of this parabola *bc* cuts at right angles the diameter *ik* of the circle *ibkc*, and the axis *ad* is parallel to the side *lk*; now having taken any point *f* in the curve *bfa* draw the straight line *fe* parallel to *bd*; then, I say, the square of *bd* is to the square of *fe* in the same ratio as the axis *ad* is to the portion *ae*.



We can now resume the text and see how the Author demonstrates his first proposition in which he shows that a body falling with a motion compounded of a uniform horizontal and a naturally accelerated one describes a semi-parabola.

Let us imagine an elevated horizontal line or plane *ab* along which a body moves with uniform speed from *a* to *b*. Suppose this plane to end abruptly at *b*; then at this point the body will, on account of its weight, acquire also a natural motion downwards along the perpendicular *bn*. Draw the line *be* along the plane *ba* to represent the flow, or measure, of time; divide this line into a number of segments, *bc, cd, de,* representing equal intervals of time; from the points *b, c, d, e,* let fall lines which are parallel to the perpendicular *bn*. On the first of these lay off any distance *ci,* on the second a distance four times as long, *df;* on the third, one nine times as long, *eh;* and so on, in proportion to the squares of *cb, db, eb,* or, we may say, in the squared ratio of these same lines. Accordingly we see that while the body moves from *b* to *c* with uniform speed, it also falls perpendicularly through the distance *ci,* and at the end of the time-interval *bc* finds itself at the point *i*. In like manner at the end of the time-interval *bd,* which is the double of *bc,* the vertical fall will be four times the first distance *ci;* for it has been shown in a previous discussion that the distance traversed by a freely falling body varies as the square of the time; in like manner the space *eh* traversed

during the time *be* will be nine times *ci;* thus it is evident that the distances *eh, df, ci* will be to one another as the squares of the lines *be, bd, bc.* Now from the points *i, f, h* draw the straight lines *io, fg, hl* parallel to *be;* these lines *hl, fg, io* are equal to *eb, db* and *cb,* respectively; so also are the lines *bo, bg, bl* respectively equal to *ci, df,* and *eh.* The square of *hl* is to that of *fg* as the line *lb* is to *bg;* and the square of *fg* is to that of *io* as *gb* is to *bo;* therefore the points *i, f, h,* lie on one and the same parabola. In like manner it may be shown that, if we take equal time-intervals of any size whatever, and if we imagine the particle to be carried by a similar compound motion, the positions of this particle, at the ends of these time-intervals, will lie on one and the same parabola. Q.E.D.



This conclusion follows the converse of the first of the two propositions given above. For, having drawn a parabola through the points *b* and *h,* any other two points, *f* and *i,* not falling on the parabola must lie either within or without; consequently the line *fg* is either longer or shorter than the line which terminates on the parabola. Therefore the square of *hl* will not bear to the square of *fg* the same ratio as the line *lb* to *bg,* but a greater or smaller; the fact is, however, that the square of *hl* does bear this same ratio to the square of *fg.* Hence the point *f* does lie on the parabola, and so do all the others.

SAGREDO. One cannot deny that the argument is new, subtle and conclusive, resting as it does upon this hypothesis, namely, that the horizontal motion remains uniform, that the vertical motion continues to be accelerated downwards in proportion to the square of the time, and that such motions and velocities as these combine without altering, disturbing, or hindering each other, so that as the motion proceeds the path of the projectile does not change into a different curve: but this, in my opinion, is impossible. For the axis of the parabola along which we imagine the natural motion of a falling body to take place stands perpendicular to a horizontal surface and ends at the center of the earth; and since the parabola deviates more and more from its axis no projectile can ever reach the center of the earth or, if it does, as seems necessary, then the path of the projectile must transform itself into some other curve very different from the parabola.

SIMPLICIO. To these difficulties, I may add others. One of these is that we suppose the horizontal plane, which slopes neither up nor down, to be represented by a straight line as if each point on this line were equally distant from the center, which is not the case; for as one starts from the middle [of the line] and goes toward either end, he departs farther and farther from the center [of the earth] and is therefore constantly going uphill. Whence it follows that the motion cannot remain uniform through any distance whatever, but must continually diminish. Besides, I do not see how it is possible to avoid the resistance of the medium which must destroy the uniformity of the horizontal motion and change the law of acceleration of falling bodies. These various difficulties render it highly improbable that a result derived from such unreliable hypotheses should hold true in practice.

SALVIATI. All these difficulties and objections which you urge are so well founded that it is impossible to remove them; and, as for me, I am ready to admit them all, which indeed I think our Author would also do. I grant that these conclusions proved in the abstract will be different when applied in the concrete and will be fallacious to this extent, that neither will the horizontal motion be uniform nor the natural acceleration be in the ratio assumed, nor the path of the projectile a parabola, etc. But, on the other hand, I ask you not to begrudge our Author that which other eminent men have assumed even if not strictly true. The authority of Archimedes alone will satisfy everybody. In his *Mechanics* and in his first quadrature of the parabola he takes for granted that the beam of a balance or steelyard is a straight line, every point of which is equidistant from the common center of all heavy bodies, and that the cords by which heavy bodies are suspended are parallel to each other.

Some consider this assumption permissible because, in practice, our instruments and the distances involved are so small in comparison with the enormous distance from the center of the earth that we may consider a minute of arc on a great circle as a straight line, and may regard the perpendiculars let fall from its two extremities as parallel. For if in actual practice one had to consider such small quantities, it would be necessary first of all to criticise the architects who presume, by use of a plumb line, to erect high towers with parallel sides. I may add that, in all their discussions, Archimedes and the others considered themselves as located at an infinite distance from the center of the earth, in which case their assumptions were not false, and therefore their conclusions were absolutely correct. When we wish to apply our proven conclusions to distances which, though finite, are very large, it is necessary for us to infer, on the basis of demonstrated truth, what correction is to be made for the fact that our distance from the center of the earth is not really infinite, but merely very great in comparison with the small dimensions of our apparatus. The largest of these will be the range of our projectiles—and even here we need consider only the artillery—which, however great, will never exceed four of those miles of which as many thousand separate us from the center of the earth; and since these paths terminate upon the surface of the earth only very slight

changes can take place in their parabolic figure which, it is conceded, would be greatly altered if they terminated at the center of the earth.

As to the perturbation arising from the resistance of the medium this is more considerable and does not, on account of its manifold forms, submit to fixed laws and exact description. Thus if we consider only the resistance which the air offers to the motions studied by us, we shall see that it disturbs them all and disturbs them in an infinite variety of ways corresponding to the infinite variety in the form, weight, and velocity of the projectiles. For as to velocity, the greater this is, the greater will be the resistance offered by the air; a resistance which will be greater as the moving bodies become less dense. So that although the falling body ought to be displaced in proportion to the square of the duration of its motion, yet no matter how heavy the body, if it falls from a very considerable height, the resistance of the air will be such as to prevent any increase in speed and will render the motion uniform; and in proportion as the moving body is less dense this uniformity will be so much the more quickly attained and after a shorter fall. Even horizontal motion which, if no impediment were offered, would be uniform and constant is altered by the resistance of the air and finally ceases; and here again the less dense the body the quicker the process. Of these properties of weight, of velocity, and also of form, infinite in number, it is not possible to give any exact description; hence, in order to handle this matter in a scientific way, it is necessary to cut loose from these difficulties; and having discovered and demonstrated the theorems, in the case of no resistance, to use them and apply them with such limitations as experience will teach. And the advantage of this method will not be small; for the material and shape of the projectile may be chosen, as dense and round as possible, so that it will encounter the least resistance in the medium. Nor will the spaces and velocities in general be so great but that we shall be easily able to correct them with precision.

In the case of those projectiles which we use, made of dense material and round in shape, or of lighter material and cylindrical in shape, such as arrows, thrown from a sling or crossbow, the deviation from an exact parabolic path is quite insensible. Indeed, if you will allow me a little greater liberty, I can show you, by two experiments, that the dimensions of our apparatus are so small that these external and incidental resistances, among which that of the medium is the most considerable, are scarcely observable.

I now proceed to the consideration of motions through the air, since it is with these that we are now especially concerned; the resistance of the air exhibits itself in two ways: first by offering greater impedance to less dense than to very dense bodies, and secondly by offering greater resistance to a body in rapid motion than to the same body in slow motion.

Regarding the first of these, consider the case of two balls having the same dimensions, but one weighing ten or twelve times as much as

the other; one, say, of lead, the other of oak, both allowed to fall from an elevation of 150 or 200 cubits.

Experiment shows that they will reach the earth with slight difference in speed, showing us that in both cases the retardation caused by the air is small; for if both balls start at the same moment and at the same elevation, and if the leaden one be slightly retarded and the wooden one greatly retarded, then the former ought to reach the earth a considerable distance in advance of the latter, since it is ten times as heavy. But this does not happen; indeed, the gain in distance of one over the other does not amount to the hundredth part of the entire fall. And in the case of a ball of stone weighing only a third or half as much as one of lead, the difference in their times of reaching the earth will be scarcely noticeable. Now since the speed acquired by a leaden ball in falling from a height of 200 cubits is so great that if the motion remained uniform the ball would, in an interval of time equal to that of the fall, traverse 400 cubits, and since this speed is so considerable in comparison with those which, by use of bows or other machines except firearms, we are able to give to our projectiles, it follows that we may, without sensible error, regard as absolutely true those propositions which we are about to prove without considering the resistance of the medium.

Passing now to the second case, where we have to show that the resistance of the air for a rapidly moving body is not very much greater than for one moving slowly, ample proof is given by the following experiment. Attach to two threads of equal length—say four or five yards—two equal leaden balls and suspend them from the ceiling; now pull them aside from the perpendicular, the one through 80 or more degrees, the other through not more than four or five degrees; so that, when set free, the one falls, passes through the perpendicular, and describes large but slowly decreasing arcs of 160, 150, 140 degrees, etc.; the other swinging through small and also slowly diminishing arcs of 10, 8, 6 degrees, etc.

In the first place it must be remarked that one pendulum passes through its arcs of 180°, 160°, etc., in the same time that the other swings through its 10°, 8°, etc., from which it follows that the speed of the first ball is 16 and 18 times greater than that of the second. Accordingly, if the air offers more resistance to the high speed than to the low, the frequency of vibration in the large arcs of 180° or 160°, etc., ought to be less than in the small arcs of 10°, 8°, 4°, etc., and even less than in arcs of 2°, or 1°; but this prediction is not verified by experiment; because if two persons start to count the vibrations, the one the large, the other the small, they will discover that after counting tens and even hundreds they will not differ by a single vibration, not even by a fraction of one.

This observation justifies the two following propositions, namely, that vibrations of very large and very small amplitude all occupy the same time and that the resistance of the air does not affect motions of high speed more than those of low speed, contrary to the opinion hitherto generally entertained.

SAGREDO. On the contrary, since we cannot deny that the air hinders

both of these motions, both becoming slower and finally vanishing, we have to admit that the retardation occurs in the same proportion in each case. But how? How, indeed, could the resistance offered to the one body be greater than that offered to the other except by the impartation of more momentum and speed to the fast body than to the slow? And if this is so the speed with which a body moves is at once the cause and measure of the resistance which it meets. Therefore, all motions, fast or slow, are hindered and diminished in the same proportion; a result, it seems to me, of no small importance.

SALVIATI. We are able, therefore, in this second case to say that the errors, neglecting those which are accidental, in the results which we are about to demonstrate are small in the case of our machines where the velocities employed are mostly very great and the distances negligible in comparison with the semi-diameter of the earth or one of its great circles.

SIMPLICIO. I would like to hear your reason for putting the projectiles of firearms, i. e., those using powder, in a different class from the projectiles employed in bows, slings, and crossbows, on the ground of their not being equally subject to change and resistance from the air.

SALVIATI. I am led to this view by the excessive and, so to speak, supernatural violence with which such projectiles are launched; for, indeed, it appears to me that without exaggeration one might say that the speed of a ball fired either from a musket or from a piece of ordnance is supernatural. For if such a ball be allowed to fall from some great elevation its speed will, owing to the resistance of the air, not go on increasing indefinitely; that which happens to bodies of small density in falling through short distances—I mean the reduction of their motion to uniformity—will also happen to a ball of iron or lead after it has fallen a few thousand cubits; this terminal or final speed is the maximum which such a heavy body can naturally acquire in falling through the air. This speed I estimate to be much smaller than that impressed upon the ball by the burning powder.

An appropriate experiment will serve to demonstrate this fact. From a height of one hundred or more cubits fire a gun loaded with a lead bullet vertically downwards upon a stone pavement; with the same gun shoot against a similar stone from a distance of one or two cubits, and observe which of the two balls is the more flattened. Now if the ball which has come from the greater elevation is found to be the less flattened of the two, this will show that the air has hindered and diminished the speed initially imparted to the bullet by the powder, and that the air will not permit a bullet to acquire so great a speed, no matter from what height it falls; for if the speed impressed upon the ball by the fire does not exceed that acquired by it in falling freely then its downward blow ought to be greater rather than less.

This experiment I have not performed, but I am of the opinion that a musket ball or cannon shot, falling from a height as great as you please, will not deliver so strong a blow as it would if fired into a wall only a few cubits distant, i. e., at such a short range that the splitting or rending

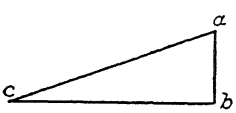
of the air will not be sufficient to rob the shot of that excess of supernatural violence given it by the powder.

The enormous momentum of these violent shots may cause some deformation of the trajectory, making the beginning of the parabola flatter and less curved than the end; but, so far as our Author is concerned, this is a matter of small consequence in practical operations, the main one of which is the preparation of a table of ranges for shots of high elevation, giving the distance attained by the ball as a function of the angle of elevation; and since shots of this kind are fired from mortars using small charges and imparting no supernatural momentum they follow their prescribed paths very exactly.

But now let us proceed with the discussion in which the Author invites us to the study and investigation of the motion of a body when that motion is compounded of two others; and first the case in which the two are uniform, the one horizontal, the other vertical.

Theorem II, Proposition II

When the motion of a body is the resultant of two uniform motions, one horizontal, the other perpendicular, the square of the resultant momentum is equal to the sum of the squares of the two component momenta.



SIMPLICIO. At this point there is just one slight difficulty which needs to be cleared up; for it seems to me that the conclusion just reached contradicts a previous proposition in which it is claimed that the speed of a body coming from *a* to *b* is equal to that in coming from *a* to *c;* while now you conclude that the speed at *c* is greater than that at *b*.

SALVIATI. Both propositions, Simplicio, are true, yet there is a great difference between them. Here we are speaking of a body urged by a single motion which is the resultant of two uniform motions, while there we were speaking of two bodies each urged with naturally accelerated motions, one along the vertical *ab* the other along the inclined plane *ac*. Besides the time-intervals were there not supposed to be equal, that along the incline *ac* being greater than that along the vertical *ab;* but the motions of which we now speak, those along *ab, bc, ac,* are uniform and simultaneous.

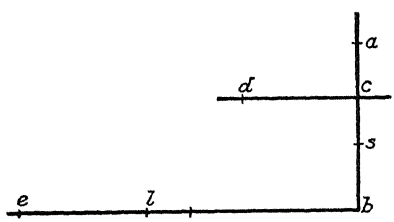
SIMPLICIO. Pardon me; I am satisfied; pray go on.

SALVIATI. Our Author next undertakes to explain what happens when a body is urged by a motion compounded of one which is horizontal and uniform and of another which is vertical but naturally accelerated; from these two components results the path of a projectile, which is a parab-

ola. The problem is to determine the speed of the projectile at each point. With this purpose in view our Author sets forth as follows the manner, or rather the method, of measuring such speed along the path which is taken by a heavy body starting from rest and falling with a naturally accelerated motion.

Theorem III, Proposition III

Let the motion take place along the line *ab*, starting from rest at *a*, and in this line choose any point *c*. Let *ac* represent the time, or the measure of the time, required for the body to fall through the space *ac;* let *ac* also represent the velocity at *c* acquired by a fall through the distance *ac*. In the line *ab* select any other point *b*. The problem now is to determine the velocity at *b* acquired by a body in falling through the distance *ab* and to express this in terms of the velocity at *c*, the measure of which is the length *ac*. Take *as* a mean proportional between *ac* and



ab. We shall prove that the velocity at *b* is to that at *c* as the length *as* is to the length *ac*. Draw the horizontal line *cd*, having twice the length of *ac*, and *be*, having twice the length of *ba*. It then follows, from the preceding theorems, that a body falling through the distance *ac*, and turned so as to move along the horizontal *cd* with a uniform speed equal to that acquired on reaching *c*, will traverse the distance *cd* in the same interval of time as that required to fall with accelerated motion from *a* to *c*. Likewise *be* will be traversed in the same time as *ba*. But the time of descent through *ab* is *as;* hence the horizontal distance *be* is also traversed in the time *as*. Take a point *l* such that the time *as* is to the time *ac* as *be* is to *bl;* since the motion along *be* is uniform, the distance *bl*, if traversed with the speed acquired at *b*, will occupy the time *ac;* but in this same time-interval, *ac*, the distance *cd* is traversed with the speed acquired in *c*. Now two speeds are to each other as the distances traversed in equal intervals of time. Hence the speed at *c* is to the speed at *b* as *cd* is to *bl*. But since *dc* is to *be* as their halves, namely, as *ca* is to *ba*, and since *be* is to *bl* as *ba* is to *sa;* it follows that *dc* is to *bl* as *ca* is to *sa*. In other words, the speed at *c* is to that at *b* as *ca* is to *sa*, that is, as the time to fall through *ab*.

The method of measuring the speed of a body along the direction of its fall is thus clear; the speed is assumed to increase directly as the time.

Problem. Proposition IV

SALVIATI. Concerning motions and their velocities or momenta whether uniform or naturally accelerated, one cannot speak definitely until he has established a measure for such velocities and also for time. As for time we have the already widely adopted hours, first minutes and second minutes. So for velocities, just as for intervals of time, there is need of a common standard which shall be understood and accepted by everyone, and which shall be the same for all. As has already been stated, the Author considers the velocity of a freely falling body adapted to this purpose, since this velocity increases according to the same law in all parts of the world; thus for instance the speed acquired by a leaden ball of a pound weight starting from rest and falling vertically through the height of, say, a spear's length is the same in all places; it is therefore excellently adapted for representing the momentum acquired in the case of natural fall.

It still remains for us to discover a method of measuring momentum in the case of uniform motion in such a way that all who discuss the subject will form the same conception of its size and velocity. This will prevent one person from imagining it larger, another smaller, than it really is; so that in the composition of a given uniform motion with one which is accelerated different men may not obtain different values for the resultant. In order to determine and represent such a momentum and particular speed our Author has found no better method than to use the momentum acquired by a body in naturally accelerated motion. The speed of a body which has in this manner acquired any momentum whatever will, when converted into uniform motion, retain precisely such a speed as, during a time-interval equal to that of the fall, will carry the body through a distance equal to twice that of the fall. But since this matter is one which is fundamental in our discussion it is well that we make it perfectly clear by means of some particular example.

Let us consider the speed and momentum acquired by a body falling through the height, say, of a spear as a standard which we may use in the measurement of other speeds and momenta as occasion demands; assume for instance that the time of such a fall is four seconds; now in order to measure the speed acquired from a fall through any other height, whether greater or less, one must not conclude that these speeds bear to one another the same ratio as the heights of fall; for instance, it is not true that a fall through four times a given height confers a speed four times as great as that acquired by descent through the given height; because the speed of a naturally accelerated motion does not vary in proportion to the time. As has been shown above, the ratio of the spaces is equal to the square of the ratio of the times.

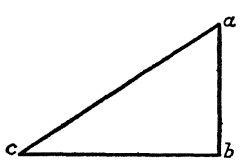
If, then, as is often done for the sake of brevity, we take the same limited straight line as the measure of the speed, and of the time, and

also of the space traversed during that time, it follows that the duration of fall and the speed acquired by the same body in passing over any other distance, is not represented by this second distance, but by a mean proportional between the two distances. This I can better illustrate by an example. In the vertical line *ac,* lay off the portion *ab* to represent the distance traversed by a body falling freely with accelerated motion: the time of fall may be represented by any limited straight line, but for the sake of brevity, we shall represent it by the same length *ab;* this length may also be employed as a measure of the momentum and speed acquired during the motion; in short, let *ab* be a measure of the various physical quantities which enter this discussion.

Having agreed arbitrarily upon *ab* as a measure of these three different quantities, namely, space, time, and momentum, our next task is to find the time required for fall through a given vertical distance *ac,* also the momentum acquired at the terminal point *c,* both of which are to be expressed in terms of the time and momentum represented by *ab*. These two required quantities are obtained by laying off *ad;* a mean proportional between *ab* and *ac;* in other words, the time of fall from *a* to *c* is represented by *ad* on the same scale on which we agreed that the time of fall from *a* to *b* should be represented by *ab*. In like manner we may say that the momentum acquired at *c* is related to that acquired at *b,* in the same manner that the line *ad* is related to *ab,* since the velocity varies directly as the time, a conclusion which, although employed as a postulate in Proposition III, is here amplified by the Author.

This point being clear and well-established we pass to the consideration of the momentum in the case of two compound motions, one of which is compounded of a uniform horizontal and a uniform vertical motion, while the other is compounded of a uniform horizontal and a naturally accelerated vertical motion. If both components are uniform, and one at right angles to the other, we have already said that the square of the resultant is obtained by adding the squares of the components as will be clear from the following illustration.

Let us imagine a body to move along the vertical *ab* with a uniform momentum of 3, and on reaching *b* to move toward *c* with a momentum



of 4, so that during the same time-interval it will traverse 3 cubits along the vertical and 4 along the horizontal. But a particle which moves with the resultant velocity will, in the same time, traverse the diagonal *ac,* whose length is not 7 cubits—the sum of *ab* (3) and *bc* (4)—but 5, which

is *in potenza* equal to the sum of 3 and 4; that is, the squares of 3 and 4 when added make 25, which is the square of *ac,* and is equal to the sum of the squares of *ab* and *bc*. Hence *ac* is represented by the side—or we may say the root—of a square whose area is 25, namely 5.

As a fixed and certain rule for obtaining the momentum which results from two uniform momenta, one vertical, the other horizontal, we have therefore the following: take the square of each, add these together, and extract the square root of the sum, which will be the momentum resulting from the two. Thus, in the above example, the body which in virtue of its vertical motion would strike the horizontal plane with a momentum of 3, would owing to its horizontal motion alone strike at *c* with a momentum of 4; but if the body strikes with a momentum which is the resultant of these two, its blow will be that of a body moving with a momentum of 5; and such a blow will be the same at all points of the diagonal *ac,* since its components are always the same and never increase or diminish.

Let us now pass to the consideration of a uniform horizontal motion compounded with the vertical motion of a freely falling body starting from rest. It is at once clear that the diagonal which represents the motion compounded of these two is not a straight line, but, as has been demonstrated, a semi-parabola, in which the momentum is always increasing because the speed of the vertical component is always increasing. Wherefore, to determine the momentum at any given point in the parabolic diagonal, it is necessary first to fix upon the uniform horizontal momentum and then, treating the body as one falling freely, to find the vertical momentum at the given point; this latter can be determined only by taking into account the duration of fall, a consideration which does not enter into the composition of two uniform motions where the velocities and momenta are always the same; but here where one of the component motions has an initial value of zero and increases its speed in direct proportion to the time, it follows that the time must determine the speed at the assigned point. It only remains to obtain the momentum resulting from these two components (as in the case of uniform motions) by placing the square of the resultant equal to the sum of the squares of the two components.

To what has hitherto been said concerning the momenta, blows or shocks of projectiles, we must add another very important consideration; to determine the force and energy of the shock it is not sufficient to consider only the speed of the projectiles, but we must also take into account the nature and condition of the target which, in no small degree, determines the efficiency of the blow. First of all it is well known that the target suffers violence from the speed of the projectile in proportion as it partly or entirely stops the motion; because if the blow falls upon an object which yields to the impulse without resistance such a blow will be of no effect; likewise when one attacks his enemy with a spear and overtakes him at an instant when he is fleeing with equal speed there will be no blow but merely a harmless touch. But if the shock falls upon an

object which yields only in part then the blow will not have its full effect, but the damage will be in proportion to the excess of the speed of the projectile over that of the receding body; thus, for example, if the shot reaches the target with a speed of 10 while the latter recedes with a speed of 4, the momentum and shock will be represented by 6. Finally the blow will be a maximum, in so far as the projectile is concerned, when the target does not recede at all but if possible completely resists and stops the motion of the projectile. I have said *in so far as the projectile is concerned* because if the target should approach the projectile the shock of collision would be greater in proportion as the sum of the two speeds is greater than that of the projectile alone.

Moreover it is to be observed that the amount of yielding in the target depends not only upon the quality of the material, as regards hardness, whether it be of iron, lead, wool, etc., but also upon its position. If the position is such that the shot strikes it at right angles, the momentum imparted by the blow will be a maximum; but if the motion be oblique, that is to say slanting, the blow will be weaker; and more and more so in proportion to the obliquity; for, no matter how hard the material of the target thus situated, the entire momentum of the shot will not be spent and stopped; the projectile will slide by and will, to some extent, continue its motion along the surface of the opposing body.

All that has been said above concerning the amount of momentum in the projectile at the extremity of the parabola must be understood to refer to a blow received on a line at right angles to this parabola or along the tangent to the parabola at the given point; for, even though the motion has two components, one horizontal, the other vertical, neither will the momentum along the horizontal nor that upon a plane perpendicular to the horizontal be a maximum, since each of these will be received obliquely.

SAGREDO. Your having mentioned these blows and shocks recalls to my mind a problem, or rather a question, in mechanics of which no author has given a solution or said anything which diminishes my astonishment or even partly relieves my mind.

My difficulty and surprise consist in not being able to see whence and upon what principle is derived the energy and immense force which makes its appearance in a blow; for instance we see the simple blow of a hammer, weighing not more than 8 or 10 lbs., overcoming resistances which, without a blow, would not yield to the weight of a body producing impetus by pressure alone, even though that body weighed many hundreds of pounds. I would like to discover a method of measuring the force of such a percussion. I can hardly think it infinite, but incline rather to the view that it has its limit and can be counterbalanced and measured by other forces, such as weights, or by levers or screws or other mechanical instruments which are used to multiply forces in a manner which I satisfactorily understand.

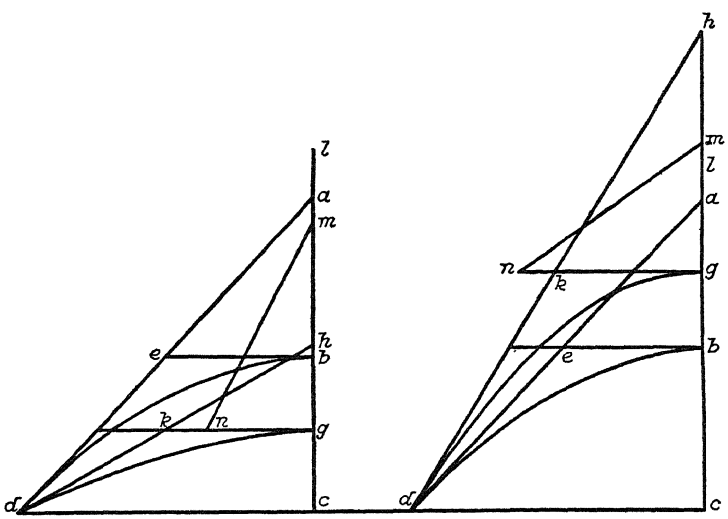
SALVIATI. You are not alone in your surprise at this effect or in obscurity as to the cause of this remarkable property. I studied this mat-

ter myself for a while in vain; but my confusion merely increased until finally meeting our Academician I received from him great consolation. First he told me that he also had for a long time been groping in the dark; but later he said that, after having spent some thousands of hours in speculating and contemplating thereon, he had arrived at some notions which are far removed from our earlier ideas and which are remarkable for their novelty. And since now I know that you would gladly hear what these novel ideas are I shall not wait for you to ask but promise that, as soon as our discussion of projectiles is completed, I will explain all these fantasies, or if you please, vagaries, as far as I can recall them from the words of our Academician. In the meantime we proceed with the propositions of the Author.

Theorem. Proposition V.

If projectiles describe semi-parabolas of the same amplitude, the momentum required to describe that one whose amplitude is double its altitude is less than that required for any other.

Let *bd* be a semi-parabola whose amplitude *cd* is double its altitude *cb;* on its axis extended upwards lay off *ba* equal to its altitude *bc*. Draw the line *ad* which will be a tangent to the parabola at *d* and will cut the



horizontal line *be* at the point *e*, making *be* equal to *bc* and also to *ba*. It is evident that this parabola will be described by a projectile whose uniform horizontal momentum is that which it would acquire at *b* in falling from rest at *a* and whose naturally accelerated vertical momentum

is that of the body falling to *c*, from rest at *b*. From this it follows that the momentum at the terminal point *d*, compounded of these two, is represented by the diagonal *ae*, whose square is equal to the sum of the squares of the two components. Now let *gd* be any other parabola whatever having the same amplitude *cd*, but whose altitude *cg* is either greater or less than the altitude *bc*. Let *hd* be the tangent cutting the horizontal through *g* at *k*. Select a point *l* such that $hg:gk=gk:gl$. Then from a preceding proposition, it follows that *gl* will be the height from which a body must fall in order to describe the parabola *gd*.

Let *gm* be a mean proportional between *ab* and *gl;* then *gm* will represent the time and momentum acquired at *g* by a fall from *l;* for *ab* has been assumed as a measure of both time and momentum. Again let *gn* be a mean proportional between *bc* and *cg;* it will then represent the time and momentum which the body acquires at *c* in falling from *g*. If now we join *m* and *n*, this line *mn* will represent the momentum at *d* of the projectile traversing the parabola *dg;* which momentum is, I say, greater than that of the projectile travelling along the parabola *bd* whose measure was given by *ae*. For since *gn* has been taken as a mean proportional between *bc* and *gc;* and since *bc* is equal to *be* and also to *kg* (each of them being the half of *dc*) it follows that $cg:gn=gn:gk$, and as *cg* or (*hg*) is to *gk* so is ng^2 to gk^2: but by construction $hg:gk=gk:gl$. Hence $ng^2:gk^2=gk:gl$. But $gk:gl=gk^2:gm^2$, since *gm* is a mean proportional between *kg* and *gl*. Therefore the three squares *ng, kg, mg* form a continued proportion, $gn^2:gk^2=gk^2:gm^2$. And the sum of the two extremes which is equal to the square of *mn* is greater than twice the square of *gk;* but the square of *ae* is double the square of *gk*. Hence the square of *mn* is greater than the square of *ae* and the length *mn* is greater than the length *ae*. Q.E.D.

Corollary

Conversely it is evident that less momentum will be required to send a projectile from the terminal point *d* along the parabola *bd* than along any other parabola having an elevation greater or less than that of the parabola *bd*, for which the tangent at *d* makes an angle of 45° with the horizontal. From which it follows that if projectiles are fired from the terminal point *d*, all having the same speed, but each having a different elevation, the maximum range, i. e., amplitude of the semi-parabola or of the entire parabola, will be obtained when the elevation is 45°: the other shots, fired at angles greater or less, will have a shorter range.

SAGREDO. The force of rigid demonstrations such as occur only in mathematics fills me with wonder and delight. From accounts given by gunners, I was already aware of the fact that in the use of cannon and mortars, the maximum range, that is, the one in which the shot goes farthest, is obtained when the elevation is 45° or, as they say, at the sixth point of the quadrant; but to understand why this happens far outweighs

the mere information obtained by the testimony of others or even by repeated experiment.

SALVIATI. What you say is very true. The knowledge of a single fact acquired through a discovery of its causes prepares the mind to understand and ascertain other facts without need of recourse to experiment, precisely as in the present case, where by argumentation alone the Author proves with certainty that the maximum range occurs when the elevation is 45°. He thus demonstrates what has perhaps never been observed in experience, namely, that of other shots those which exceed or fall short of 45° by equal amounts have equal ranges; so that if the balls have been fired one at an elevation of 7 points, the other at 5, they will strike the level at the same distance: the same is true if the shots are fired at 8 and at 4 points, at 9 and at 3, etc.

SIMPLICIO. I am fully satisfied. So now Salviati can present the speculations of our Academician on the subject of impulsive forces.

SALVIATI. Let the preceding discussions suffice for today; the hour is already late and the time remaining will not permit us to clear up the subjects proposed; we may therefore postpone our meeting until another and more opportune occasion.

SAGREDO. I concur in your opinion, because after various conversations with intimate friends of our Academician I have concluded that this question of impulsive forces is very obscure, and I think that, up to the present, none of those who have treated this subject have been able to clear up its dark corners which lie almost beyond the reach of human imagination; among the various views which I have heard expressed one, strangely fantastic, remains in my memory, namely, that impulsive forces are indeterminate, if not infinite. Let us, therefore, await the convenience of Salviati.

END OF FOURTH DAY

PRINCIPIA

The Mathematical Principles of Natural Philosophy

by

ISAAC NEWTON

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ISAAC NEWTON

1642–1727

THE most familiar story about Isaac Newton concerns his curiosity about a falling apple and his consequent discovery of the law of gravity. This story, first recorded by Voltaire, who had it from Newton's favorite niece, may be true. It is at least not improbable; for Newton from an early age habitually observed natural phenomena closely, constantly asked "Why?" and constantly tried to set his explanations in mathematical forms.

Born on Christmas Day in 1642, the posthumous son of a freehold farmer at Woolsthorpe in Lincolnshire, Newton had his early education in small schools in his neighborhood. In 1654 he entered the grammar school at Grantham, six miles away. When he graduated from this school at the top of his class, he had, like his schoolmates, built kites and water clocks and dials; he had also contrived a four-wheeled carriage to be propelled by the occupant, and had made marked progress in mathematics. Yet when he came home to his mother—now the widow of Barnabas Smith, a clergyman —at Woolsthorpe, no one thought of any career for him but that of a small farmer. He engaged in ordinary farm routine, performed chores, went to market with his mother's agent. The agent reported that on market days the boy spent his time at bookstalls. He was frequently observed poring over mathematical treatises. Eventually his mother's brother, the rector of a parish near by, and himself a graduate of Trinity College, Cambridge, persuaded the widow Smith that her son should also go to Trinity. He was entered as a subsizar in 1661.

At Cambridge, Newton showed that he had already mastered Sanderson's *Logic,* and that, scorning Euclid as too easy to be worth studying, he had gone deep into Descartes's *Geometry*. His low opinion of Euclid he later revised; but not until after he had mastered Wallis's *Arithmetic of Infinites.*

As an undergraduate, he did make series of observations on natural phenomena such as the moon's halo, but his genius was for mathematics. In 1665 he discovered what is now known as the binomial theorem, and a little later, the elements of the differential calculus, which he called "fluxions." When, in 1668, he took his master's degree at Trinity, of which he was now a fellow, he wrote a paper which attracted the attention of the chief mathematicians of England. The following year his friend and teacher, Barrow, resigned as Lucasian professor of mathematics at Cambridge, and Newton was appointed to succeed him.

As Lucasian professor, Newton was required to lecture once a week on some portion of geometry, arithmetic, astronomy, geography, optics, statics, or other mathematical subject, and to receive students two hours a week. Choosing optics as his first topic, and later other subjects in mathematics, he lectured regularly until 1701, when he resigned his professorship. His lectures on algebra were published in 1707 by his successor in the Lucasian chair, Whiston, under the title *Arithmetica Universalis*. Other unpublished lectures may be of equal merit. Yet these years were surely productive less of great lectures than of great papers for the Royal Society.

To the Society, Newton had early sent a paper commenting on a reflecting telescope of his own invention. So well was it received that he sent other papers, several of them the developed forms of ideas and discoveries really dating from his student days. In 1672, after the Royal Society had elected him to membership, there was read to it Newton's "New Theory about Light and Color," the paper in which he reported his discovery of the composition of white light. An immense controversy ensued, for Hooke, among the eminent English scientists, and Lucas and Linus, among the continental scientists, were only three of many men who violently denied the plausibility of Newton's announcement. He quietly stood his ground—content that experiment rather than argument should prove him right.

Many of Newton's papers for the Society—reports on polarization, on double refraction, on binocular vision, and so on—are now obsolete. One of them, however, developed his emission, or corpuscular, theory of light which contemporary physicists have been seriously reconsidering. And another, "De Motu," contained the germ of the *Principia*.

Celestial mechanics had been fascinating to Newton for a long time. As early as 1666, when the plague closed Cambridge and sent the undergraduate Newton home to Woolsthorpe, he was considering the possibility that gravity might extend as far as the orb of the moon. Later, to explain why

the planets keep to elliptical orbits round the attracting sun, he calculated the inverse-square law. Then he applied the law to explain the path of the moon round the earth, and was dissatisfied with his computations. He convinced himself that in order to apply the law, he would first have to demonstrate mathematically that spherical bodies such as the sun and the moon act as point centers of force. By 1684, when Halley, Wren, and Hooke had all agreed on the inverse-square law—although they could not prove it—Newton had completed his calculations. He was sure now that the law applied, and he explained his solution of the great problem in "De Motu."

During the next two years Newton composed the *Principia Mathematica Philosophiae Naturalis.* In 1685 he announced his law of universal gravitation and simultaneously gave the Royal Society the first book of the *Principia.* The whole of the great work was finally published in 1687. In 1729, Andrew Motte published the first English translation; from the 1803 edition of this translation the following passages are taken.

A nervous illness—described by Pepys as "an attack of phrenitis," that is, madness—afflicted Newton in 1692. Within eighteen months he had recovered. But from the time of this illness until his death thirty-five years later, he made no great contribution to scientific knowledge. *The Opticks,* published in 1704, and Newton's only large work in English, really contains the results of studies made much earlier; and his Law of Cooling, announced to the Royal Society in 1701, he had also computed and used much earlier.

During his later years honors in abundance came to Newton. He became the president of the Royal Society in 1703, and by annual re-election held the office until his death. In 1695 he was appointed Warden of the Mint, and, in 1699, Master of the Mint. These appointments returned him many times the income he earned as Lucasian professor at Cambridge, and made possible the rather elaborate style of living he came to enjoy. Twice, in 1689 and again in 1701, he represented Cambridge as the university's member in Parliament. The French Academy made him a foreign member in 1699. In 1705, Queen Anne's consort, Prince George of Denmark, who as a member of the Royal Society greatly admired Newton and his work, persuaded the queen to knight Newton.

Unmarried, Newton seemed to enjoy equally the pleasures of London, of the Cambridge cloisters, and of his estate at Woolsthorpe. Gradually he gave more and more attention to matters not wholly scientific. He compiled a *Chronology of Ancient Kingdoms* (1728), wrote theological treatises such as *Observations on the Prophecies of Daniel,* and a *Church His-*

tory. Though his health declined as he aged, and though he suffered much from stone and gout, his mind retained such acuteness that all mathematicians deferred to him and England acknowledged him as her greatest living scientist.

The *Principia* has been for two centuries recognized as one of the world's great books. In it Newton not only sums up his own researches, but, to support them, magnificently taps the experimental and theoretical work of all the physical scholars of his and preceding times. He states definitively the first two laws of motion and adds a third, the result of his own labors; he presents and proves his Law of Universal Gravitation (see Book I, Section XII, and Book III, Proposition VIII); he shows that mass and weight are proportional to each other at any given spot on the earth (Book II, Proposition XXIV); he deduces the velocity of sound, explains the tides, traces the paths of comets, demonstrates that the sun is the center of our system, and so on.

To make the calculations upon which his generalizations rest, Newton frequently used "fluxions"—what we call calculus. But though he suggests his method in Book I, Lemmae I, II, and XI, he did not fully explain his new method until he presented it formally in 1693, in the third volume of Dr. Wallis's works. Rather, in the *Principia,* he presents everything in the Euclidean manner. From a small number of axioms he proceeds to a series of mathematical—generally geometrical—propositions and demonstrations. Thus, like Euclid and Archimedes, he moves steadily, logically, relentlessly, from the known and acknowledged to the new and surprising. As a result, until Planck announced the Quantum Theory in 1900, Newton's conclusions controlled all physical thinking; and the validity of the *Principia* remains unchallenged today within the area of gross mechanics. It is Newton's monument.

(Since terminology has changed in two centuries, the contemporary reader needs to be aware that Newton's terms must be understood as follows: *subducted,* subtracted; *conjunctly,* cross multiplied; *congress,* impact; *invention,* discovery; *used to be,* are usually; *observed the duplicate ratio,* vary as the square; *in the duplicate ratio,* as the square; *in the triplicate ratio,* as the cube; *in the sesquiplicate ratio,* as the $3/2$ power; *in the subduplicate ratio,* as the square root; *in the subtriplicate ratio,* as the cube root. Thus, in modern terminology, Book One, Section Two, Proposition IV, Corollary 2 will read: "And since the periodic times are as the radii divided by the velocities; the centripetal forces are as the radii divided by the square of the periodic times.")

PRINCIPIA

The Mathematical Principles of Natural Philosophy

DEFINITIONS

DEFINITION I

The quantity of matter is the measure of the same, arising from its density and bulk conjunctly.

THUS air of a double density, in a double space, is quadruple in quantity; in a triple space, sextuple in quantity. The same thing is to be understood of snow, and fine dust or powders, that are condensed by compression or liquefaction; and of all bodies that are by any causes whatever differently condensed. I have no regard in this place to a medium, if any such there is, that freely pervades the interstices between the parts of bodies. It is this quantity that I mean hereafter everywhere under the name of body or mass. And the same is known by the weight of each body; for it is proportional to the weight, as I have found by experiments on pendulums, very accurately made, which shall be shewn hereafter.

DEFINITION II

The quantity of motion is the measure of the same, arising from the velocity and quantity of matter conjunctly.

The motion of the whole is the sum of the motions of all the parts; and therefore in a body double in quantity, with equal velocity, the motion is double; with twice the velocity, it is quadruple.

DEFINITION III

The vis insita, *or innate force of matter, is a power of resisting, by which every body, as much as in it lies, endeavours to persevere in its pres-*

to, and detains them in their orbits, which I therefore call centripetal, would fly off in right lines, with an uniform motion. A projectile, if it was not for the force of gravity, would not deviate towards the earth, but would go off from it in a right line, and that with an uniform motion, if the resistance of the air was taken away. It is by its gravity that it is drawn aside perpetually from its rectilinear course, and made to deviate towards the earth, more or less, according to the force of its gravity, and the velocity of its motion. The less its gravity is, for the quantity of its matter, or the greater the velocity with which it is projected, the less will it deviate from a rectilinear course, and the farther it will go. If a leaden ball, projected from the top of a mountain by the force of gunpowder with a given velocity, and in a direction parallel to the horizon, is carried in a curve line to the distance of two miles before it falls to the ground; the same, if the resistance of the air were taken away, with a double or decuple velocity, would fly twice or ten times as far. And by increasing the velocity, we may at pleasure increase the distance to which it might be projected, and diminish the curvature of the line, which it might describe, till at last it should fall at the distance of 10, 30, or 90 degrees, or even might go quite round the whole earth before it falls; or lastly, so that it might never fall to the earth, but go forward into the celestial spaces, and proceed in its motion *in infinitum*. And after the same manner that a projectile, by the force of gravity, may be made to revolve in an orbit, and go round the whole earth, the moon also, either by the force of gravity, if it is endued with gravity, or by any other force, that impels it towards the earth, may be perpetually drawn aside towards the earth, out of the rectilinear way, which by its innate force it would pursue; and would be made to revolve in the orbit which it now describes; nor could the moon, without some such force, be retained in its orbit. If this force was too small, it would not sufficiently turn the moon out of a rectilinear course: if it was too great, it would turn it too much, and draw down the moon from its orbit towards the earth. It is necessary, that the force be of a just quantity, and it belongs to the mathematicians to find the force, that may serve exactly to retain a body in a given orbit, with a given velocity; and *vice versa,* to determine the curvilinear way, into which a body projected from a given place, with a given velocity, may be made to deviate from its natural rectilinear way, by means of a given force.

The quantity of any centripetal force may be considered as of three kinds; absolute, accelerative, and motive.

DEFINITION VI

The absolute quantity of a centripetal force is the measure of the same proportional to the efficacy of the cause that propagates it from the centre, through the spaces round about.

Thus the magnetic force is greater in one loadstone and less in another according to their sizes and strength of intensity.

DEFINITION VII

The accelerative quantity of a centripetal force is the measure of the same, proportional to the velocity which it generates in a given time.

Thus the force of the same loadstone is greater at a less distance, and less at a greater: also the force of gravity is greater in valleys, less on tops of exceeding high mountains; and yet less (as shall hereafter be shown) at greater distances from the body of the earth; but at equal distances, it is the same everywhere; because (taking away, or allowing for, the resistance of the air), it equally accelerates all falling bodies, whether heavy or light, great or small.

DEFINITION VIII

The motive quantity of a centripetal force is the measure of the same, proportional to the motion which it generates in a given time.

Thus the weight is greater in a greater body, less in a less body; and, in the same body, it is greater near to the earth, and less at remoter distances. This sort of quantity is the centripetency, or propension of the whole body towards the centre, or, as I may say, its weight; and it is always known by the quantity of an equal and contrary force just sufficient to hinder the descent of the body.

These quantities of forces, we may, for brevity's sake, call by the names of motive, accelerative, and absolute forces; and, for distinction's sake, consider them, with respect to the bodies that tend to the centre; to the places of those bodies; and to the centre of force towards which they tend; that is to say, I refer the motive force to the body as an endeavour and propensity of the whole towards a centre, arising from the propensities of the several parts taken together; the accelerative force to the place of the body, as a certain power or energy diffused from the centre to all places around to move the bodies that are in them; and the absolute force to the centre, as endued with some cause, without which those motive forces would not be propagated through the spaces round about; whether that cause be some central body (such as is the loadstone, in the centre of the magnetic force, or the earth in the centre of the gravitating force), or anything else that does not yet appear. For I here design only to give a mathematical notion of those forces, without considering their physical causes and seats.

Wherefore the accelerative force will stand in the same relation to the motive, as celerity does to motion. For the quantity of motion arises from the celerity drawn into the quantity of matter; and the motive force arises from the accelerative force drawn into the same quantity of matter. For the sum of the actions of the accelerative force, upon the several particles of the body, is the motive force of the whole. Hence it is, that near the

surface of the earth, where the accelerative gravity, or force productive of gravity, in all bodies is the same, the motive gravity or the weight is as the body: but if we should ascend to higher regions, where the accelerative gravity is less, the weight would be equally diminished, and would always be as the product of the body, by the accelerative gravity. So in those regions, where the accelerative gravity is diminished into one half, the weight of a body two or three times less will be four or six times less.

I likewise call attractions and impulses, in the same sense, accelerative, and motive; and use the words attraction, impulse or propensity of any sort towards a centre, promiscuously, and indifferently, one for another; considering those forces not physically, but mathematically: wherefore, the reader is not to imagine, that by those words, I anywhere take upon me to define the kind, or the manner of any action, the causes or the physical reason thereof, or that I attribute forces, in a true and physical sense, to certain centres (which are only mathematical points); when at any time I happen to speak of centres as attracting, or as endued with attractive powers.

SCHOLIUM

Hitherto I have laid down the definitions of such words as are less known, and explained the sense in which I would have them to be understood in the following discourse. I do not define time, space, place and motion, as being well known to all. Only I must observe, that the vulgar conceive those quantities under no other notions but from the relation they bear to sensible objects. And thence arise certain prejudices, for the removing of which, it will be convenient to distinguish them into absolute and relative, true and apparent, mathematical and common.

I. Absolute, true, and mathematical time, of itself, and from its own nature, flows equably without regard to anything external, and by another name is called duration: relative, apparent, and common time is some sensible and external (whether accurate or unequable) measure of duration by the means of motion, which is commonly used instead of true time; such as an hour, a day, a month, a year.

II. Absolute space, in its own nature, without regard to anything external, remains always similar and immovable. Relative space is some movable dimension or measure of the absolute spaces; which our senses determine by its position to bodies; and which is vulgarly taken for immovable space; such is the dimension of a subterraneous, an æreal, or celestial space, determined by its position in respect of the earth. Absolute and relative space are the same in figure and magnitude; but they do not remain always numerically the same. For if the earth, for instance, moves, a space of our air, which relatively and in respect of the earth remains always the same, will at one time be one part of the absolute space into which the air passes; at another time it will be another part of the same, and so, absolutely understood, it will be perpetually mutable.

III. Place is a part of space which a body takes up, and is, according to the space, either absolute or relative. I say, a part of space; not the situation, nor the external surface of the body. For the places of equal solids are always equal; but their superficies, by reason of their dissimilar figures, are often unequal. Positions properly have no quantity, nor are they so much the places themselves, as the properties of places. The motion of the whole is the same thing with the sum of the motions of the parts; that is, the translation of the whole, out of its place, is the same thing with the sum of the translations of the parts out of their places; and therefore the place of the whole is the same thing with the sum of the places of the parts, and for that reason, it is internal, and in the whole body.

IV. Absolute motion is the translation of a body from one absolute place into another; and relative motion, the translation from one relative place into another. Thus in a ship under sail, the relative place of a body is that part of the ship which the body possesses; or that part of its cavity which the body fills, and which therefore moves together with the ship: and relative rest is the continuance of the body in the same part of the ship, or of its cavity. But real, absolute rest is the continuance of the body in the same part of that immovable space, in which the ship itself, its cavity, and all that it contains, is moved. Wherefore, if the earth is really at rest, the body, which relatively rests in the ship, will really and absolutely move with the same velocity which the ship has on the earth. But if the earth also moves, the true and absolute motion of the body will arise, partly from the true motion of the earth, in immovable space; partly from the relative motion of the ship on the earth; and if the body moves also relatively in the ship its true motion will arise, partly from the true motion of the earth, in immovable space, and partly from the relative motions as well of the ship on the earth, as of the body in the ship; and from these relative motions will arise the relative motion of the body on the earth. As if that part of the earth, where the ship is, was truly moved toward the east, with a velocity of 10010 parts; while the ship itself, with a fresh gale, and full sails, is carried towards the west, with a velocity expressed by 10 of those parts; but a sailor walks in the ship towards the east, with 1 part of the said velocity; then the sailor will be moved truly in immovable space towards the east, with a velocity of 10001 parts, and relatively on the earth towards the west, with a velocity of 9 of those parts.

Absolute time, in astronomy, is distinguished from relative, by the equation or correction of the vulgar time. For the natural days are truly unequal, though they are commonly considered as equal, and used for a measure of time; astronomers correct this inequality for their more accurate deducing of the celestial motions. It may be that there is no such thing as an equable motion, whereby time may be accurately measured. All motions may be accelerated and retarded, but the true, or equable, progress of absolute time is liable to no change. The duration or perseverance of the existence of things remains the same, whether the motions are swift or slow, or none at all: and therefore it ought to be distinguished

from what are only sensible measures thereof; and out of which we collect it, by means of the astronomical equation. The necessity of which equation, for determining the times of a phænomenon, is evinced as well from the experiments of the pendulum clock, as by eclipses of the satellites of *Jupiter*.

As the order of the parts of time is immutable, so also is the order of the parts of space. Suppose those parts to be moved out of their places, and they will be moved (if the expression may be allowed) out of themselves. For times and spaces are, as it were, the places as well of themselves as of all other things. All things are placed in time as to order of succession; and in space as to order of situation. It is from their essence or nature that they are places; and that the primary places of things should be movable is absurd. These are therefore the absolute places; and translations out of those places are the only absolute motions.

But because the parts of space cannot be seen, or distinguished from one another by our senses, therefore in their stead we use sensible measures of them. For from the positions and distances of things from any body considered as immovable, we define all places; and then with respect to such places, we estimate all motions, considering bodies as transferred from some of those places into others. And so, instead of absolute places and motions, we use relative ones; and that without any inconvenience in common affairs; but in philosophical disquisitions, we ought to abstract from our senses, and consider things themselves, distinct from what are only sensible measures of them. For it may be that there is no body really at rest, to which the places and motions of others may be referred.

But we may distinguish rest and motion, absolute and relative, one from the other by their properties, causes and effects. It is a property of rest, that bodies really at rest do rest in respect to one another. And therefore as it is possible, that in the remote regions of the fixed stars, or perhaps far beyond them, there may be some body absolutely at rest; but impossible to know, from the position of bodies to one another in our regions, whether any of these do keep the same position to that remote body; it follows that absolute rest cannot be determined from the position of bodies in our regions.

It is a property of motion, that the parts, which retain given positions to their wholes, do partake of the motions of those wholes. For all the parts of revolving bodies endeavour to recede from the axis of motion; and the impetus of bodies moving forward arises from the joint impetus of all the parts. Therefore, if surrounding bodies are moved, those that are relatively at rest within them will partake of their motion. Upon which account, the true and absolute motion of a body cannot be determined by the translation of it from those which only seem to rest; for the external bodies ought not only to appear at rest, but to be really at rest. For otherwise, all included bodies, beside their translation from near the surrounding ones, partake likewise of their true motions; and though that translation were not made they would not be really at rest, but only seem to be so. For the surrounding bodies stand in the like relation to the

surrounded as the exterior part of a whole does to the interior, or as the shell does to the kernel; but, if the shell moves, the kernel will also move, as being part of the whole, without any removal from near the shell.

A property, near akin to the preceding, is this, that if a place is moved, whatever is placed therein moves along with it; and therefore a body, which is moved from a place in motion, partakes also of the motion of its place. Upon which account, all motions, from places in motion, are no other than parts of entire and absolute motions; and every entire motion is composed of the motion of the body out of its first place, and the motion of this place out of its place; and so on, until we come to some immovable place, as in the before-mentioned example of the sailor. Wherefore, entire and absolute motions can be no otherwise determined than by immovable places; and for that reason I did before refer those absolute motions to immovable places, but relative ones to movable places. Now no other places are immovable but those that, from infinity to infinity, do all retain the same given position one to another; and upon this account must ever remain unmoved; and do thereby constitute immovable space.

The causes by which true and relative motions are distinguished, one from the other, are the forces impressed upon bodies to generate motion. True motion is neither generated nor altered, but by some force impressed upon the body moved; but relative motion may be generated or altered without any force impressed upon the body. For it is sufficient only to impress some force on other bodies with which the former is compared, that by their giving way, that relation may be changed, in which the relative rest or motion of this other body did consist. Again, true motion suffers always some change from any force impressed upon the moving body; but relative motion does not necessarily undergo any change by such forces. For if the same forces are likewise impressed on those other bodies, with which the comparison is made, that the relative position may be preserved, then that condition will be preserved in which the relative motion consists. And therefore any relative motion may be changed when the true motion remains unaltered, and the relative may be preserved when the true suffers some change. Upon which accounts, true motion does by no means consist in such relations.

The effects which distinguish absolute from relative motion are the forces of receding from the axis of circular motion. For there are no such forces in a circular motion purely relative, but in a true and absolute circular motion they are greater or less, according to the quantity of the motion. If a vessel, hung by a long cord, is so often turned about that the cord is strongly twisted, then filled with water, and held at rest together with the water; after, by the sudden action of another force, it is whirled about the contrary way, and while the cord is untwisting itself, the vessel continues for some time in this motion; the surface of the water will at first be plain, as before the vessel began to move; but the vessel, by gradually communicating its motion to the water, will make it begin sensibly to revolve, and recede by little and little from the middle, and ascend to the sides of the vessel, forming itself into a concave figure (as I have ex-

perienced), and the swifter the motion becomes, the higher will the water rise, till at last, performing its revolutions in the same times with the vessel, it becomes relatively at rest in it. This ascent of the water shows its endeavour to recede from the axis of its motion; and the true and absolute circular motion of the water, which is here directly contrary to the relative, discovers itself, and may be measured by this endeavour. At first, when the relative motion of the water in the vessel was greatest, it produced no endeavour to recede from the axis; the water showed no tendency to the circumference, nor any ascent towards the sides of the vessel, but remained of a plain surface, and therefore its true circular motion had not yet begun. But afterwards, when the relative motion of the water had decreased, the ascent thereof towards the sides of the vessel proved its endeavour to recede from the axis; and this endeavour showed the real circular motion of the water perpetually increasing, till it had acquired its greatest quantity, when the water rested relatively in the vessel. And therefore this endeavour does not depend upon any translation of the water in respect of the ambient bodies, nor can true circular motion be defined by such translation. There is only one real circular motion of any one revolving body, corresponding to only one power of endeavouring to recede from its axis of motion, as its proper and adequate effect; but relative motions, in one and the same body, are innumerable, according to the various relations it bears to external bodies, and, like other relations, are altogether destitute of any real effect, any otherwise than they may perhaps partake of that one only true motion. And therefore in their system who suppose that our heavens, revolving below the sphere of the fixed stars, carry the planets along with them; the several parts of those heavens, and the planets, which are indeed relatively at rest in their heavens, do yet really move. For they change their position one to another (which never happens to bodies truly at rest), and being carried together with their heavens, partake of their motions, and as parts of revolving wholes, endeavour to recede from the axis of their motions.

Wherefore relative quantities are not the quantities themselves, whose names they bear, but those sensible measures of them (either accurate or inaccurate), which are commonly used instead of the measured quantities themselves. And if the meaning of words is to be determined by their use, then by the names time, space, place and motion, their measures are properly to be understood; and the expression will be unusual, and purely mathematical, if the measured quantities themselves are meant. Upon which account, they do strain the sacred writings, who there interpret those words for the measured quantities. Nor do those less defile the purity of mathematical and philosophical truths, who confound real quantities themselves with their relations and vulgar measures.

It is indeed a matter of great difficulty to discover, and effectually to distinguish, the true motions of particular bodies from the apparent; because the parts of that immovable space, in which those motions are performed, do by no means come under the observation of our senses. Yet the thing is not altogether desperate; for we have some arguments to

guide us, partly from the apparent motions, which are the differences of the true motions; partly from the forces, which are the causes and effects of the true motions. For instance, if two globes, kept at a given distance one from the other by means of a cord that connects them, were revolved about their common centre of gravity, we might, from the tension of the cord, discover the endeavour of the globes to recede from the axis of their motion, and from thence we might compute the quantity of their circular motions. And then if any equal forces should be impressed at once on the alternate faces of the globes to augment or diminish their circular motions, from the increase or decrease of the tension of the cord, we might infer the increment or decrement of their motions; and thence would be found on what faces those forces ought to be impressed, that the motions of the globes might be most augmented; that is, we might discover their hindermost faces, or those which, in the circular motion, do follow. But the faces which follow being known, and consequently the opposite ones that precede, we should likewise know the determination of their motions. And thus we might find both the quantity and the determination of this circular motion, even in an immense vacuum, where there was nothing external or sensible with which the globes could be compared. But now, if in that space some remote bodies were placed that kept always a given position one to another, as the fixed stars do in our regions, we could not indeed determine, from the relative translation of the globes among those bodies, whether the motion did belong to the globes or to the bodies. But if we observed the cord, and found that its tension was that very tension which the motions of the globes required, we might conclude the motion to be in the globes, and the bodies to be at rest; and then, lastly, from the translation of the globes among the bodies, we should find the determination of their motions. But how we are to collect the true motions from their causes, effects, and apparent differences; and, *vice versa,* how from the motions, either true or apparent, we may come to the knowledge of their causes and effects, shall be explained more at large in the following tract. For to this end it was that I composed it.

AXIOMS, OR LAWS OF MOTION

LAW I

Every body perseveres in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed thereon.

Projectiles persevere in their motions, so far as they are not retarded by the resistance of the air, or impelled downwards by the force of gravity. A top, whose parts by their cohesion are perpetually drawn aside from rectilinear motions, does not cease its rotation, otherwise than as it is retarded by the air. The greater bodies of the planets and comets, meeting

with less resistance in more free spaces, preserve their motions both progressive and circular for a much longer time.

LAW II

The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.

If any force generates a motion, a double force will generate double the motion, a triple force triple the motion, whether that force be impressed altogether and at once, or gradually and successively. And this motion (being always directed the same way with the generating force), if the body moved before, is added to or subducted from the former motion, according as they directly conspire with or are directly contrary to each other; or obliquely joined, when they are oblique, so as to produce a new motion compounded from the determination of both.

LAW III

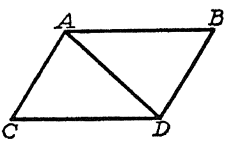
To every action there is always opposed an equal reaction: or the mutual actions of two bodies upon each other are always equal, and directed to contrary parts.

Whatever draws or presses another is as much drawn or pressed by that other. If you press a stone with your finger, the finger is also pressed by the stone. If a horse draws a stone tied to a rope, the horse (if I may so say) will be equally drawn back towards the stone: for the distended rope, by the same endeavour to relax or unbend itself, will draw the horse as much towards the stone, as it does the stone towards the horse, and will obstruct the progress of the one as much as it advances that of the other. If a body impinge upon another, and by its force change the motion of the other, that body also (because of the equality of the mutual pressure) will undergo an equal change, in its own motion, towards the contrary part. The changes made by these actions are equal, not in the velocities but in the motions of bodies; that is to say, if the bodies are not hindered by any other impediments. For, because the motions are equally changed, the changes of the velocities made towards contrary parts are reciprocally proportional to the bodies. This law takes place also in attractions.

COROLLARY I

A body by two forces conjoined will describe the diagonal of a parallelogram, in the same time that it would describe the sides, by those forces apart.

If a body in a given time, by the force M impressed apart in the place A, should with an uniform motion be carried from A to B; and by the force N impressed apart in the same place, should be carried from A to C; complete the parallelogram ABCD, and, by both forces acting together, it will in the same time be carried in the diagonal from A to D. For since the force N acts in the direction of the line AC, parallel to BD, this force (by the second law) will not at all alter the velocity generated by the other force M, by which the body is carried towards the line BD. The body therefore will arrive at the line BD in the same time, whether the force N be impressed or not; and therefore at the end of that time it



will be found somewhere in the line BD. By the same argument, at the end of the same time it will be found somewhere in the line CD. Therefore it will be found in the point D, where both lines meet. But it will move in a right line from A to D, by Law I.

COROLLARY II

And hence is explained the composition of any one direct force AD, out of any two oblique forces AC and CD; and, on the contrary, the resolution of any one direct force AD into two oblique forces AC and CD: which composition and resolution are abundantly confirmed from mechanics.

COROLLARY III

The quantity of motion, which is collected by taking the sum of the motions directed towards the same parts, and the difference of those that are directed to contrary parts, suffers no change from the action of bodies among themselves.

For action and its opposite re-action are equal, by Law III, and therefore, by Law II, they produce in the motions equal changes towards opposite parts. Therefore if the motions are directed towards the same parts, whatever is added to the motion of the preceding body will be subducted from the motion of that which follows; so that the sum will be the same as before. If the bodies meet, with contrary motions, there will be an equal deduction from the motions of both; and therefore the difference of the motions directed towards opposite parts will remain the same.

Thus if a spherical body A with two parts of velocity is triple of a spherical body B which follows in the same right line with ten parts of

velocity, the motion of A will be to that of B as 6 to 10. Suppose, then, their motions to be of 6 parts and of 10 parts, and the sum will be 16 parts. Therefore, upon the meeting of the bodies, if A acquire 3, 4, or 5 parts of motion, B will lose as many; and therefore after reflexion A will proceed with 9, 10, or 11 parts, and B with 7, 6, or 5 parts; the sum remaining always of 16 parts as before. If the body A acquire 9, 10, 11, or 12 parts of motion, and therefore after meeting proceed with 15, 16, 17, or 18 parts, the body B, losing so many parts as A has got, will either proceed with 1 part, having lost 9, or stop and remain at rest, as having lost its whole progressive motion of 10 parts; or it will go back with 1 part, having not only lost its whole motion, but (if I may so say) one part more; or it will go back with 2 parts, because a progressive motion of 12 parts is taken off. And so the sums of the conspiring motions 15+1, or 16+0, and the differences of the contrary motions 17—1 and 18—2, will always be equal to 16 parts, as they were before the meeting and reflexion of the bodies. But, the motions being known with which the bodies proceed after reflexion, the velocity of either will be also known, by taking the velocity after to the velocity before reflexion, as the motion after is to the motion before. As in the last case, where the motion of the body A was of 6 parts before reflexion and of 18 parts after, and the velocity was of 2 parts before reflexion, the velocity thereof after reflexion will be found to be of 6 parts; by saying, as the 6 parts of motion before to 18 parts after, so are 2 parts of velocity before reflexion to 6 parts after.

But if the bodies are either not spherical, or, moving in different right lines, impinge obliquely one upon the other, and their motions after reflexion are required, in those cases we are first to determine the position of the plane that touches the concurring bodies in the point of concourse, then the motion of each body (by Corol. II) is to be resolved into two, one perpendicular to that plane, and the other parallel to it. This done, because the bodies act upon each other in the direction of a line perpendicular to this plane, the parallel motions are to be retained the same after reflexion as before; and to the perpendicular motions we are to assign equal changes towards the contrary parts; in such manner that the sum of the conspiring and the difference of the contrary motions may remain the same as before. From such kind of reflexions also sometimes arise the circular motions of bodies about their own centres. But these are cases which I do not consider in what follows; and it would be too tedious to demonstrate every particular that relates to this subject.

COROLLARY IV

The common centre of gravity of two or more bodies does not alter its state of motion or rest by the actions of the bodies among themselves; and therefore the common centre of gravity of all bodies acting upon each other (excluding outward actions and impediments) is either at rest or moves uniformly in a right line.

COROLLARY V

The motions of bodies included in a given space are the same among themselves, whether that space is at rest or moves uniformly forwards in a right line without any circular motion.

For the differences of the motions tending towards the same parts, and the sums of those that tend towards contrary parts, are, at first (by supposition), in both cases the same; and it is from those sums and differences that the collisions and impulses do arise with which the bodies mutually impinge one upon another. Wherefore (by Law II), the effects of those collisions will be equal in both cases; and therefore the mutual motions of the bodies among themselves in the one case will remain equal to the mutual motions of the bodies among themselves in the other. A clear proof of which we have from the experiment of a ship; where all motions happen after the same manner, whether the ship is at rest or is carried uniformly forwards in a right line.

COROLLARY VI

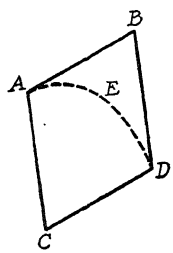
If bodies, any how moved among themselves, are urged in the direction of parallel lines by equal accelerative forces, they will all continue to move among themselves, after the same manner as if they had been urged by no such forces.

For these forces acting equally (with respect to the quantities of the bodies to be moved), and in the direction of parallel lines, will (by Law II) move all the bodies equally (as to velocity), and therefore will never produce any change in the positions or motions of the bodies among themselves.

SCHOLIUM

Hitherto I have laid down such principles as have been received by mathematicians, and are confirmed by abundance of experiments. By the first two Laws and the first two Corollaries, Galileo discovered that the descent of bodies observed the duplicate ratio of the time, and that the motion of projectiles was in the curve of a parabola; experience agreeing with both, unless so far as these motions are a little retarded by the resistance of the air. When a body is falling, the uniform force of its gravity, acting equally, impresses, in equal particles of time, equal forces upon that body, and therefore generates equal velocities; and in the whole time impresses a whole force, and generates a whole velocity proportional to the time. And the spaces described in proportional times are as the velocities and the times conjunctly; that is, in a duplicate ratio of the

times. And when a body is thrown upwards, its uniform gravity impresses forces and takes off velocities proportional to the times; and the times of ascending to the greatest heights are as the velocities to be taken off, and those heights are as the velocities and the times conjunctly, or in the duplicate ratio of the velocities. And if a body be projected in any direction, the motion arising from its projection is compounded with the motion arising from its gravity. As if the body A by its motion of projection alone could describe in a given time the right line AB, and with its motion of falling alone could describe in the same time the altitude AC; complete the parallelogram ABDC, and the body by that compounded motion will at the end of the time be found in the place D; and the curve line AED, which that body describes, will be a parabola, to



which the right line AB will be a tangent in A; and whose ordinate BD will be as the square of the line AB. On the same Laws and Corollaries depend those things which have been demonstrated concerning the times of the vibration of pendulums, and are confirmed by the daily experiments of pendulum clocks. By the same, together with the third Law, Sir Christ. Wren, Dr. Wallis, and Mr. Huygens, the greatest geometers of our times, did severally determine the rules of the congress and reflexion of hard bodies, and much about the same time communicated their discoveries to the Royal Society, exactly agreeing among themselves as to those rules. Dr. Wallis, indeed, was something more early in the publication; then followed Sir Christopher Wren, and, lastly, Mr. Huygens. But Sir Christopher Wren confirmed the truth of the thing before the Royal Society by the experiment of pendulums, which Mr. Mariotte soon after thought fit to explain in a treatise entirely upon that subject.

Book One: Of the Motion of Bodies

SECTION ONE

Of the method of first and last ratios of quantities, by the help whereof we demonstrate the propositions that follow.

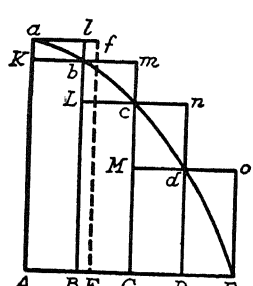
LEMMA I

Quantities, and the ratios of quantities, which in any finite time converge continually to equality, and before the end of that time approach nearer the one to the other than by any given difference, become ultimately equal.

If you deny it, suppose them to be ultimately unequal, and let D be their ultimate difference. Therefore they cannot approach nearer to equality than by that given difference D; which is against the supposition.

LEMMA II

If in any figure AacE, *terminated by the right lines* Aa, AE, *and the curve* acE, *there be inscribed any number of parallelograms* Ab, Bc, Cd, &c., *comprehended under equal bases* AB, BC, CD, &c., *and the sides,*



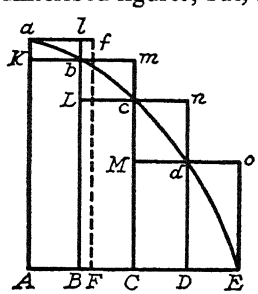
Bb, Cc, Dd, &c., *parallel to one side* Aa *of the figure; and the parallelograms* aKbl, bLcm, cMdn, &c., *are completed. Then if the breadth of those parallelograms be supposed to be diminished, and their number to be augmented* in infinitum; *I say, that the ultimate ratios which the inscribed figure* AKbLcMdD, *the circumscribed figure* Aalbmcndoe, *and curvilinear figure* AabcdE *will have to one another are ratios of equality.*

For the difference of the inscribed and circumscribed figures is the sum of the parallelograms K*l*, L*m*, M*n*, D*o*, that is (from the equality of all their bases), the rectangle under one of their bases K*b* and the sum of their altitudes A*a*, that is, the rectangle AB*la*. But this rectangle, because its breadth AB is supposed diminished *in infinitum*, becomes less than any given space. And therefore (by Lem. I) the figures inscribed and circumscribed become ultimately equal one to the other; and much more will the intermediate curvilinear figure be ultimately equal to either. Q.E.D.

LEMMA III

The same ultimate ratios are also ratios of equality, when the breadths, AB, BC, DC, &c., *of the parallelograms are unequal, and are all diminished* in infinitum.

For suppose AF equal to the greatest breadth, and complete the parallelogram FA*af*. This parallelogram will be greater than the difference of the inscribed and circumscribed figures; but, because its breadth AF is



diminished *in infinitum*, it will become less than any given rectangle. Q.E.D.

Cor. 1. Hence the ultimate sum of those evanescent parallelograms will in all parts coincide with the curvilinear figure.

Cor. 2. Much more will the rectilinear figure comprehended under the chords of the evanescent arcs *ab*, *bc*, *cd*, &c., ultimately coincide with the curvilinear figure.

Cor. 3. And also the circumscribed rectilinear figure comprehended under the tangents of the same arcs.

Cor. 4. And therefore these ultimate figures (as to their perimeters *ac*E) are not rectilinear, but curvilinear limits of rectilinear figures.

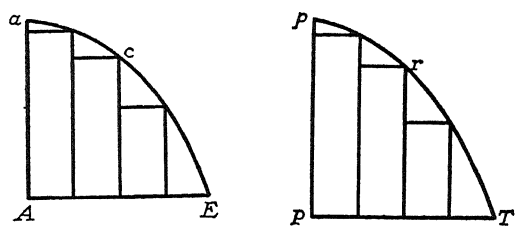
LEMMA IV

If in two figures AacE, PprT, *you inscribe (as before) two ranks of parallelograms, an equal number in each rank, and, when their breadths are diminished* in infinitum, *the ultimate ratios of the parallelograms*

in one figure to those in the other, each to each respectively, are the same; I say, that those two figures AacE, PprT, *are to one another in that same ratio.*

For as the parallelograms in the one are severally to the parallelograms in the other, so (by composition) is the sum of all in the one to the sum of all in the other; and so is the one figure to the other; because (by Lem. III) the former figure to the former sum, and the latter figure to the latter sum, are both in the ratio of equality. Q.E.D.

Cor. Hence if two quantities of any kind are any how divided into an equal number of parts, and those parts, when their number is augmented, and their magnitude diminished *in infinitum,* have a given ratio one to the other, the first to the first, the second to the second, and so on



in order, the whole quantities will be one to the other in that same given ratio. For if, in the figures of this Lemma, the parallelograms are taken one to the other in the ratio of the parts, the sum of the parts will always be as the sum of the parallelograms; and therefore supposing the number of the parallelograms and parts to be augmented, and their magnitudes diminished *in infinitum,* those sums will be in the ultimate ratio of the parallelogram in the one figure to the correspondent parallelogram in the other; that is (by the supposition), in the ultimate ratio of any part of the one quantity to the correspondent part of the other.

LEMMA V

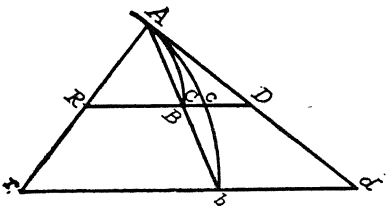
In similar figures, all sorts of homologous sides, whether curvilinear or rectilinear, are proportional· and the areas are in the duplicate ratio of the homologous sides.

LEMMA VI

If any arc ACB, *given in position, is subtended by its chord* AB, *and in any point* A, *in the middle of the continued curvature, is touched by a right line* AD, *produced both ways; then if the points* A *and* B *approach one another and meet, I say, the angle* BAD, *contained*

between the chord and the tangent, will be diminished in infinitum, *and ultimately will vanish.*

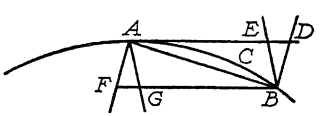
For if that angle does not vanish, the arc ACB will contain with the tangent AD an angle equal to a rectilinear angle; and therefore the curvature at the point A will not be continued, which is against the supposition.



LEMMA VII

The same things being supposed, I say that the ultimate ratio of the arc, chord, and tangent, any one to any other, is the ratio of equality.

For while the point B approaches towards the point A, consider always AB and AD as produced to the remote points *b* and *d*, and parallel to the secant BD draw *bd:* and let the arc A*cb* be always similar to the arc ACB. Then, supposing the points A and B to coincide, the angle *d*A*b* will vanish, by the preceding Lemma; and therefore the right lines A*b*, A*d* (which are always finite), and the intermediate arc A*cb*, will coincide, and become equal among themselves. Wherefore, the right lines AB, AD, and the intermediate arc ACB (which are always proportional to the former), will vanish, and ultimately acquire the ratio of equality. Q.E.D.



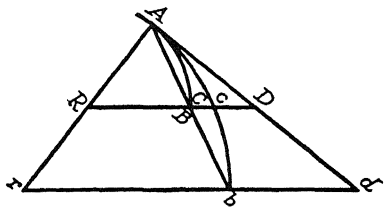
Cor. 1. Whence if through B we draw BF parallel to the tangent, always cutting any right line AF passing through A in F, this line BF will be ultimately in the ratio of equality with the evanescent arc ACB; because, completing the parallelogram AFBD, it is always in a ratio of equality with AD.

Cor. 2. And if through B and A more right lines are drawn, as BE, BD, AF, AG, cutting the tangent AD and its parallel BF; the ultimate ratio of all the abscissas AD, AE, BF, BG, and of the chord and arc AB, any one to any other, will be the ratio of equality.

Cor. 3. And therefore in all our reasoning about ultimate ratios, we may freely use any one of those lines for any other.

LEMMA VIII

If the right lines AR, BR, *with the arc* ACB, *the chord* AB, *and the tangent* AD, *constitute three triangles* RAB, RACB, RAD, *and the points* A *and* B *approach and meet: I say, that the ultimate form of these evanescent triangles is that of similitude, and their ultimate ratio that of equality.*



Cor. And hence in all reasonings about ultimate ratios, we may indifferently use any one of those triangles for any other.

LEMMA IX

If a right line AE, *and a curve line* ABC, *both given by position, cut each other in a given angle,* A; *and to that right line, in another given angle,* BD, CE *are ordinately applied, meeting the curve in* B, C; *and the points* B *and* C *together approach towards and meet in the point* A: *I say, that the areas of the triangles* ABD, ACE, *will ultimately be one to the other in the duplicate ratio of the sides.*

LEMMA X

The spaces which a body describes by any finite force urging it, whether that force is determined and immutable, or is continually augmented or continually diminished, are in the very beginning of the motion one to the other in the duplicate ratio of the times.

Cor. 1. And hence one may easily infer, that the errors of bodies describing similar parts of similar figures in proportional times are nearly as the squares of the times in which they are generated; if so be these errors are generated by any equal forces similarly applied to the bodies, and measured by the distances of the bodies from those places of the similar figures, at which, without the action of those forces, the bodies would have arrived in those proportional times.

Cor. 2. But the errors that are generated by proportional forces, similarly applied to the bodies at similar parts of the similar figures, are as the forces and the squares of the times conjunctly.

Cor. 3. The same thing is to be understood of any spaces whatsoever described by bodies urged with different forces; all which, in the very beginning of the motion, are as the forces and the squares of the times conjunctly.

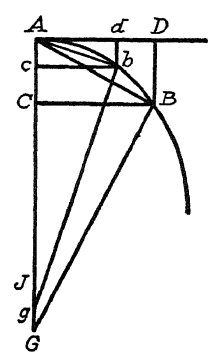
Cor. 4. And therefore the forces are as the spaces described in the very beginning of the motion directly, and the squares of the times inversely.

Cor. 5. And the squares of the times are as the spaces described directly, and the forces inversely.

LEMMA XI

The evanescent subtense of the angle of contact, in all curves which at the point of contact have a finite curvature, is ultimately in the duplicate ratio of the subtense of the conterminate arc.

Case 1. Let AB be that arc, AD its tangent, BD the subtense of the angle of contact perpendicular on the tangent, AB the subtense of the arc. Draw BG perpendicular to the subtense AB, and AG to the tangent AD, meeting in G; then let the points D, B, and G approach to the points *d, b,* and *g,* and suppose J to be the ultimate intersection of the lines BG, AG, when the points D, B, have come to A. It is evident that the distance GJ may be less than any assignable. But (from the nature of the circles passing through the points A, B, G, A, *b, g,*) $AB^2 = AG \times BD$, and $Ab^2 = Ag \times bd$; and therefore the ratio of AB^2 to Ab^2 is compounded of the ratios of AG to A*g*, and of B*d* to *bd*. But because GJ may be assumed of less length than any assignable, the ratio of AG to A*g* may be such as to differ from the ratio of equality by less than any assignable difference; and therefore the ratio of AB^2 to Ab^2 may be such as to differ from the ratio of BD to *bd* by less than any assignable difference. Therefore, by Lem. I, the ultimate ratio of AB^2 to Ab^2 is the same with the ultimate ratio of BD to *bd*. Q.E.D.



Case 2. Now let BD be inclined to AD in any given angle, and the ultimate ratio of BD to *bd* will always be the same as before, and therefore the same with the ratio of AB^2 to Ab^2. Q.E.D.

Case 3. And if we suppose the angle D not to be given, but that the right line BD converges to a given point, or is determined by any other condition whatever; nevertheless the angles D, *d,* being determined by the same law, will always draw nearer to equality, and approach nearer to each other than by any assigned difference, and therefore, by Lem. I, will at last be equal; and therefore the lines BD, *bd* are in the same ratio to each other as before. Q.E.D.

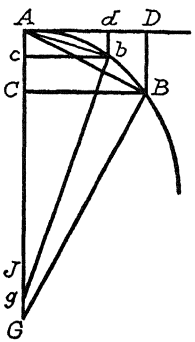
Cor. 1. Therefore since the tangents AD, A*d*, the arcs AB, A*b*, and

their sines, BC, *bc*, become ultimately equal to the chords AB, A*b*, their squares will ultimately become as the subtenses BD, *bd*.

Cor. 2. Their squares are also ultimately as the versed sines of the arcs, bisecting the chords, and converging to a given point. For those versed sines are as the subtenses BD, *bd*.

Cor. 3. And therefore the versed sine is in the duplicate ratio of the time in which a body will describe the arc with a given velocity.

Cor. 4. The rectilinear triangles ADB, A*db* are ultimately in the triplicate ratio of the sides AD, A*d*, and in a sesquiplicate ratio of the sides DB, *db*; as being in the ratio compounded of the sides AD to DB, and of A*d* to *db*. So also the triangles ABC, A*bc* are ultimately in the triplicate ratio of the sides BC, *bc*. What I call the sesquiplicate ratio is the subduplicate of the triplicate, as being compounded of the simple and subduplicate ratio.



Cor. 5. And because DB, *db* are ultimately parallel and in the duplicate ratio of the lines AD, A*d*, the ultimate curvilinear areas ADB, A*db* will be (by the nature of the parabola) two thirds of the rectilinear triangles ADB, A*db* and the segments AB, A*b* will be one third of the same triangles. And thence those areas and those segments will be in the triplicate ratio as well of the tangents AD, A*d*, as of the chords and arcs AB, A*b*.

SCHOLIUM

But we have all along supposed the angle of contact to be neither infinitely greater nor infinitely less than the angles of contact made by circles and their tangents; that is, that the curvature at the point A is neither infinitely small nor infinitely great, or that the interval AJ is of a finite magnitude. For DB may be taken as AD^3: in which case no circle can be drawn through the point A, between the tangent AD and the curve AB, and therefore the angle of contact will be infinitely less than those of circles. And by a like reasoning, if DB be made successively as AD^4, AD^5, AD^6, AD^7, &c., we shall have a series of angles of contact, proceeding *in infinitum*, wherein every succeeding term is infinitely less than the preceding. And if DB be made successively as AD^2, $AD^4/3$, $AD^4/5$, $AD^5/4$, $AD^6/5$, $AD^7/6$, &c., we shall have another infinite series of angles of contact, the first of which is of the same sort with those of circles, the second infinitely greater, and every succeeding one infinitely greater than the preceding. But between any two of these angles another series of intermediate angles of contact may be interposed, proceeding both ways *in infinitum*, wherein every succeeding angle shall be infinitely greater or infinitely less than the preceding. As if between the terms AD^2 and AD^3 there were interposed the series $AD^{1\,3}/6$, $AD^{1\,1}/5$, $AD^9/4$, $AD^7/3$, $AD^5/2$,

AD$\frac{8}{3}$, AD$1\frac{1}{4}$, AD$1\frac{4}{5}$, AD$1\frac{7}{6}$, &c. And again, between any two angles of this series, a new series of intermediate angles may be interposed, differing from one another by infinite intervals. Nor is nature confined to any bounds.

Those things which have been demonstrated of curve lines, and the superficies which they comprehend, may be easily applied to the curve superficies and contents of solids. These Lemmas are premised to avoid the tediousness of deducing perplexed demonstrations *ad absurdum,* according to the method of the ancient geometers. For demonstrations are more contracted by the method of indivisibles: but because the hypothesis of indivisibles seems somewhat harsh, and therefore that method is reckoned less geometrical, I chose rather to reduce the demonstrations of the following propositions to the first and last sums and ratios of nascent and evanescent quantities, that is, to the limits of those sums and ratios; and so to premise, as short as I could, the demonstrations of those limits. For hereby the same thing is performed as by the method of indivisibles; and now those principles being demonstrated, we may use them with more safety. Therefore if hereafter I should happen to consider quantities as made up of particles, or should use little curve lines for right ones, I would not be understood to mean indivisibles, but evanescent divisible quantities; not the sums and ratios of determinate parts, but always the limits of sums and ratios; and that the force of such demonstrations always depends on the method laid down in the foregoing Lemmas.

Perhaps it may be objected that there is no ultimate proportion of evanescent quantities; because the proportion, before the quantities have vanished, is not the ultimate, and when they are vanished, is none. But by the same argument, it may be alleged that a body arriving at a certain place, and there stopping, has no ultimate velocity: because the velocity, before the body comes to the place, is not its ultimate velocity; when it has arrived, is none. But the answer is easy; for by the ultimate velocity is meant that with which the body is moved, neither before it arrives at its last place and the motion ceases, nor after, but at the very instant it arrives; that is, that velocity with which the body arrives at its last place, and with which the motion ceases. And in like manner, by the ultimate ratio of evanescent quantities is to be understood the ratio of the quantities not before they vanish, nor afterwards, but with which they vanish. In like manner the first ratio of nascent quantities is that with which they begin to be. And the first or last sum is that with which they begin and cease to be (or to be augmented or diminished). There is a limit which the velocity at the end of the motion may attain, but not exceed. This is the ultimate velocity. And there is the like limit in all quantities and proportions that begin and cease to be. And since such limits are certain and definite, to determine the same is a problem strictly geometrical. But whatever is geometrical we may be allowed to use in determining and demonstrating any other thing that is likewise geometrical.

It may also be objected, that if the ultimate ratios of evanescent quantities are given, their ultimate magnitudes will be also given: and so

all quantities will consist of indivisibles, which is contrary to what Euclid has demonstrated concerning incommensurables, in the 10th Book of his *Elements*. But this objection is founded on a false supposition. For those ultimate ratios with which quantities vanish are not truly the ratios of ultimate quantities, but limits towards which the ratios of quantities decreasing without limit do always converge; and to which they approach nearer than by any given difference, but never go beyond, nor in effect attain to, till the quantities are diminished *in infinitum*. This thing will appear more evident in quantities infinitely great. If two quantities, whose difference is given, be augmented *in infinitum*, the ultimate ratio of these quantities will be given, to wit, the ratio of equality; but it does not from thence follow that the ultimate or greatest quantities themselves, whose ratio that is, will be given. Therefore if in what follows, for the sake of being more easily understood, I should happen to mention quantities as least, or evanescent, or ultimate, you are not to suppose that quantities of any determinate magnitude are meant, but such as are conceived to be always diminished without end.

SECTION TWO

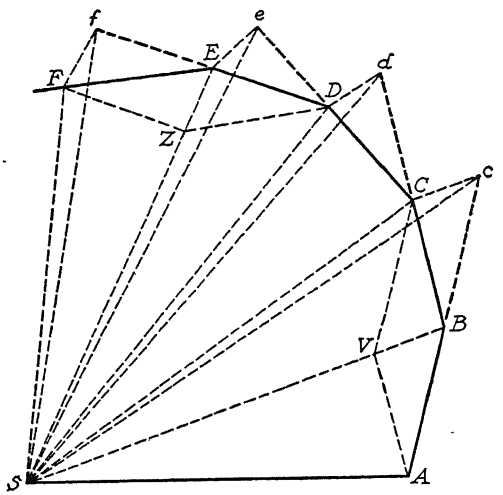
Of the invention of centripetal forces.

PROPOSITION I. THEOREM I.

The areas which revolving bodies describe by radii drawn to an immovable centre of force do lie in the same immovable planes, and are proportional to the times in which they are described.

For suppose the time to be divided into equal parts, and in the first part of that time let the body by its innate force describe the right line AB. In the second part of that time, the same would (by Law I), if not hindered, proceed directly to *c*, along the line B*c* equal to AB; so that by the radii AS, BS, *c*S, drawn to the centre, the equal areas ASB, BS*c*, would be described. But when the body is arrived at B, suppose that a centripetal force acts at once with a great impulse, and, turning aside the body from the right line B*c*, compels it afterwards to continue its motion along the right line BC. Draw *c*C parallel to BS meeting BC in C; and at the end of the second part of the time, the body (by Cor. I of the Laws) will be found in C, in the same plane with the triangle ASB. Join SC, and, because SB and C*c* are parallel, the triangle SBC will be equal to the triangle SB*c*, and therefore also to the triangle SAB. By the like argument, if the centripetal force acts successively in C, D, E, &c., and makes the body, in each single particle of time, to describe the right lines CD, DE, EF, &c., they will all lie in the same plane; and the triangle SCD will be equal to the triangle SBC, and SDE to SCD, and SEF to SDE. And therefore, in equal times, equal areas are described in one immovable plane: and, by composition, any sums SADS, SAFS, of those areas, are one to the

other as the times in which they are described. Now let the number of those triangles be augmented, and their breadth diminished *in infinitum;* and (by Cor. IV, Lem. III) their ultimate perimeter ADF will be a curve line: and therefore the centripetal force, by which the body is perpetually drawn back from the tangent of this curve will act continually; and any



described areas SADS, SAFS, which are always proportional to the times of description, will, in this case also, be proportional to those times. Q.E.D.

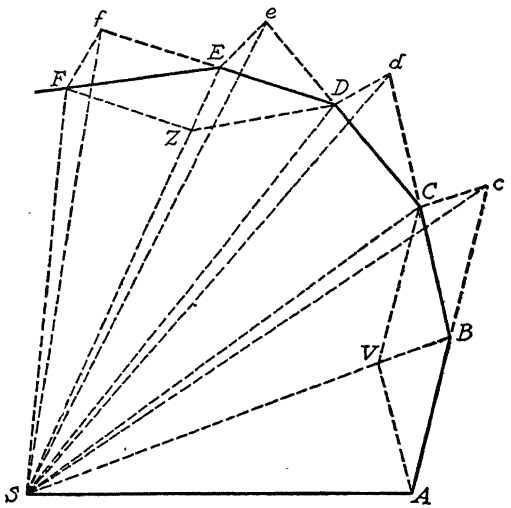
Cor. 1. The velocity of a body attracted towards an immovable centre, in spaces void of resistance, is reciprocally as the perpendicular let fall from that centre on the right line that touches the orbit. For the velocities in those places A, B, C, D, E, are as the bases AB, BC, CD, DE, EF, of equal triangles; and these bases are reciprocally as the perpendiculars let fall upon them.

Cor. 2. If the chords AB, BC of two arcs, successively described in equal times by the same body, in spaces void of resistance, are completed into a parallelogram ABCV, and the diagonal BV of this parallelogram, in the position which it ultimately acquires when those arcs are diminished *in infinitum,* is produced both ways, it will pass through the centre of force.

PROPOSITION II. THEOREM II.

Every body that moves in any curve line described in a plane, and by a radius, drawn to a point either immovable, or moving forward with an uniform rectilinear motion, describes about that point areas proportional to the times, is urged by a centripetal force directed to that point.

For every body that moves in a curve line is (by Law I) turned aside from its rectilinear course by the action of some force that impels it. And that force by which the body is turned off from its rectilinear course, and is made to describe, in equal times, the equal least triangles SAB, SBC,



SCD, &c., about the immovable point S (by Prop. XL. Book One, *Elem.* and Law II), acts in the place B, according to the direction of a line parallel to *c*C, that is, in the direction of the line BS, and in the place C, according to the direction of a line parallel to *d*D, that is, in the direction of the line CS, &c.; and therefore acts always in the direction of lines tending to the immovable point S. Q.E.D.

SCHOLIUM

A body may be urged by a centripetal force compounded of several forces; in which case the meaning of the Proposition is that the force which results out of all tends to the point S. But if any force acts perpetually in the direction of lines perpendicular to the described surface, this force will make the body to deviate from the plane of its motion: but will neither augment nor diminish the quantity of the described surface and is therefore to be neglected in the composition of forces.

PROPOSITION III. THEOREM III.

Every body that by a radius drawn to the centre of another body, howsoever moved, describes areas about that centre proportional to the times is urged by a force compounded out of the centripetal force

tending to that other body, and of all the accelerative force by which that other body is impelled.

Let L represent the one, and T the other body; and (by Cor. VI of the Laws) if both bodies are urged in the direction of parallel lines, by a new force equal and contrary to that by which the second body T is urged, the first body L will go on to describe about the other body T the same areas as before: but the force by which that other body T was urged will be now destroyed by an equal and contrary force; and therefore (by Law I) that other body T, now left to itself, will either rest or move uniformly forward in a right line: and the first body L, impelled by the difference of the forces, that is, by the force remaining, will go on to describe about the other body T areas proportional to the times. And therefore (by Theor. II) the difference of the forces is directed to the other body T as its centre. Q.E.D.

SCHOLIUM

Because the equable description of areas indicates that a centre is respected by that force with which the body is most affected, and by which it is drawn back from its rectilinear motion, and retained in its orbit; why may we not be allowed, in the following discourse, to use the equable description of areas as an indication of a centre, about which all circular motion is performed in free spaces?

PROPOSITION IV. THEOREM IV.

The centripetal forces of bodies, which by equable motions describe different circles, tend to the centres of the same circles; and are one to the other as the squares of the arcs described in equal times applied to the radii of the circles.

These forces tend to the centres of the circles (by Prop. II and Cor. II, Prop. I), and are one to another as the versed sines of the least arcs described in equal times; that is, as the squares of the same arcs applied to the diameters of the circles (by Lem. VII); and therefore since those arcs are as arcs described in any equal times, and the diameters are as the radii, the forces will be as the squares of any arcs described in the same time applied to the radii of the circles. Q.E.D.

Cor. 1. Therefore, since those arcs are as the velocities of the bodies, the centripetal forces are in a ratio compounded of the duplicate ratio of the velocities directly, and of the simple ratio of the radii inversely.

Cor. 2. And since the periodic times are in a ratio compounded of the ratio of the radii directly and the ratio of the velocities inversely, the centripetal forces are in a ratio compounded of the ratio of the radii directly and the duplicate ratio of the periodic times inversely.

Cor. 3. Whence if the periodic times are equal, and the velocities therefore as the radii, the centripetal forces will be also as the radii; and the contrary.

Cor. 4. If the periodic times and the velocities are both in the subduplicate ratio of the radii, the centripetal forces will be equal among themselves; and the contrary.

Cor. 5. If the periodic times are as the radii, and therefore the velocities equal, the centripetal forces will be reciprocally as the radii; and the contrary.

Cor. 6. If the periodic times are in the sesquiplicate ratio of the radii, and therefore the velocities reciprocally in the subduplicate ratio of the radii, the centripetal forces will be in the duplicate ratio of the radii inversely; and the contrary.

Cor. 7. And universally, if the periodic time is as any power R^n of the radius R, and therefore the velocity reciprocally as the power R^{n-1} of the radius, the centripetal force will be reciprocally as the power R^{2n-1} of the radius; and the contrary.

Cor. 8. The same things all hold concerning the times, the velocities, and forces by which bodies describe the similar parts of any similar figures that have their centres in a similar position with those figures; as appears by applying the demonstration of the preceding cases to those. And the application is easy, by only substituting the equable description of areas in the place of equable motion, and using the distances of the bodies from the centres instead of the radii.

Cor. 9. From the same demonstration it likewise follows that the arc which a body, uniformly revolving in a circle by means of a given centripetal force, describes in any time is a mean proportional between the diameter of the circle and the space which the same body falling by the same given force would descend through in the same given time.

SCHOLIUM

The case of the 6th Corollary obtains in the celestial bodies (as Sir Christopher Wren, Dr. Hooke, and Dr. Halley have severally observed); and therefore in what follows, I intend to treat more at large of those things which relate to centripetal force decreasing in a duplicate ratio of the distances from the centres.

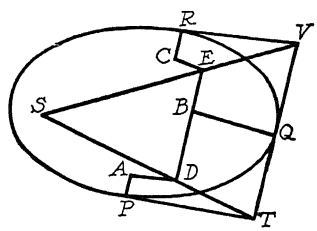
Moreover, by means of the preceding Proposition and its Corollaries, we may discover the proportion of a centripetal force to any other known force, such as that of gravity. For if a body by means of its gravity revolves in a circle concentric to the earth, this gravity is the centripetal force of that body. But from the descent of heavy bodies, the time of one entire revolution, as well as the arc described in any given time, is given (by Cor. 9 of this Prop.). And by such propositions, Mr. Huygens, in his excellent book *De Horologio Oscillatorio,* has compared the force of gravity with the centrifugal forces of revolving bodies.

The preceding Proposition may be likewise demonstrated after this manner. In any circle suppose a polygon to be inscribed of any number of sides. And if a body, moved with a given velocity along the sides of the polygon, is reflected from the circle at the several angular points, the force, with which at every reflection it strikes the circle, will be as its velocity: and therefore the sum of the forces, in a given time, will be as that velocity and the number of reflections conjunctly; that is (if the species of the polygon be given), as the length described in that given time, and increased or diminished in the ratio of the same length to the radius of the circle; that is, as the square of that length applied to the radius; and therefore the polygon, by having its sides diminished *in infinitum,* coincides with the circle, as the square of the arc described in a given time applied to the radius. This is the centrifugal force, with which the body impels the circle; and to which the contrary force, wherewith the circle continually repels the body towards the centre, is equal.

PROPOSITION V. PROBLEM I.

There being given, in any places, the velocity with which a body describes a given figure, by means of forces directed to some common centre: to find that centre.

Let the three right lines PT, TQV, VR touch the figure described in as many points, P, Q, R, and meet in T and V. On the tangents erect the perpendiculars PA, QB, RC, reciprocally proportional to the veloci-



ties of the body in the points P, Q, R, from which the perpendiculars were raised; that is, so that PA may be to QB as the velocity in Q to the velocity in P, and QB to RC as the velocity in R to the velocity in Q. Through the ends A, B, C, of the perpendiculars draw AD, DBE, EC, at right angles, meeting in D and E: and the right lines TD, VE produced, will meet in S, the centre required.

For the perpendiculars let fall from the centre S on the tangents PT, QT, are reciprocally as the velocities of the bodies in the points P and Q (by Cor. 1, Prop. I), and therefore, by construction, as the perpendiculars AP, BQ directly; that is, as the perpendiculars let fall from the point D on the tangents. Whence it is easy to infer that the points S, D, T are in one right line. And by the like argument the points S, E, V are also in one

right line; and therefore the centre S is in the point where the right lines TD, VE meet. Q.E.D.

SECTION TWELVE

Of the attractive forces of sphærical bodies.

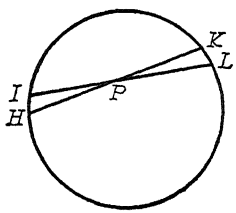
SCHOLIUM

These Propositions naturally lead us to the analogy there is between centripetal forces, and the central bodies to which those forces used to be directed; for it is reasonable to suppose that forces which are directed to bodies should depend upon the nature and quantity of those bodies, as we see they do in magnetical experiments. And when such cases occur, we are to compute the attractions of the bodies by assigning to each of their particles its proper force, and then collecting the sum of them all. I here use the word attraction in general for any endeavour, of what kind soever, made by bodies to approach to each other; whether that endeavour arise from the action of the bodies themselves, as tending mutually to or agitating each other by spirits emitted; or whether it arises from the action of the æther or of the air, or of any medium whatsoever, whether corporeal or incorporeal, any how impelling bodies placed therein towards each other. In the same general sense I use the word impulse, not defining in this treatise the species or physical qualities of forces, but investigating the quantities and mathematical proportions of them; as I observed before in the Definitions. In mathematics we are to investigate the quantities of forces with their proportions consequent upon any conditions supposed; then, when we enter upon physics, we compare those proportions with the phænomena of Nature, that we may know what conditions of those forces answer to the several kinds of attractive bodies. And this preparation being made, we argue more safely concerning the physical species, causes, and proportions of the forces. Let us see, then, with what forces sphærical bodies consisting of particles endued with attractive powers in the manner above spoken of must act mutually upon one another; and what kind of motions will follow from thence.

PROPOSITION LXX. THEOREM XXX.

If to every point of a sphærical surface there tend equal centripetal forces decreasing in the duplicate ratio of the distances from those points; I say, that a corpuscle placed within that superficies will not be attracted by those forces any way.

Let HIKL be that sphærical superficies, and P a corpuscle placed within. Through P let there be drawn to this superficies two lines HK, IL, intercepting very small arcs HI, KL; and because (by Cor. 3, Lem. VII) the triangles HPI, LPK are alike, those arcs will be proportional to the distances HP, LP; and any particles at HI and KL of the sphærical superficies, terminated by right lines passing through P, will be in the duplicate ratio of those distances. Therefore the forces of these particles exerted



upon the body P are equal between themselves. For the forces are as the particles directly, and the squares of the distances inversely. And these two ratios compose the ratio of equality. The attractions therefore, being made equally towards contrary parts, destroy each other. And by a like reasoning all the attractions through the whole sphærical superficies are destroyed by contrary attractions. Therefore the body P will not be any way impelled by those attractions. Q.E.D.

PROPOSITION LXXI. THEOREM XXXI.

The same things supposed as above, I say, that a corpuscle placed without the sphærical superficies is attracted towards the centre of the sphere with a force reciprocally proportional to the square of its distance from that centre.

PROPOSITION LXXII. THEOREM XXXII.

If to the several points of a sphere there tend equal centripetal forces decreasing in a duplicate ratio of the distances from those points; and there be given both the density of the sphere and the ratio of the diameter of the sphere to the distance of the corpuscle from its centre; I say, that the force with which the corpuscle is attracted is proportional to the semi-diameter of the sphere.

For conceive two corpuscles to be severally attracted by two spheres, one by one, the other by the other, and their distances from the centres of the spheres to be proportional to the diameters of the spheres respectively, and the spheres to be resolved into like particles, disposed in a like

situation to the corpuscles. Then the attractions of one corpuscle towards the several particles of one sphere will be to the attractions of the other towards as many analogous particles of the other sphere in a ratio compounded of the ratio of the particles directly, and the duplicate ratio of the distances inversely. But the particles are as the spheres, that is, in a triplicate ratio of the diameters, and the distances are as the diameters; and the first ratio directly with the last ratio taken twice inversely becomes the ratio of diameter to diameter. Q.E.D.

Cor. 1. Hence if corpuscles revolve in circles about spheres composed of matter equally attracting, and the distances from the centres of the spheres be proportional to their diameters, the periodic times will be equal.

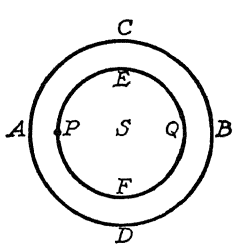
Cor. 2. And, *vice versa,* if the periodic times are equal, the distances will be proportional to the diameters. These two Corollaries appear from Cor. 3, Prop. IV.

Cor. 3. If to the several points of any two solids whatever, of like figure and equal density, there tend equal centripetal forces decreasing in a duplicate ratio of the distances from those points, the forces, with which corpuscles placed in a like situation to those two solids will be attracted by them, will be to each other as the diameters of the solids.

PROPOSITION LXXIII. THEOREM XXXIII.

If to the several points of a given sphere there tend equal centripetal forces decreasing in a duplicate ratio of the distances from the point; I say, that a corpuscle placed within the sphere is attracted by a force proportional to its distance from the centre.

In the sphere ABCD, described about the centre S, let there be placed the corpuscle P; and about the same centre S, with the interval SP, conceive described an interior sphere PEQF. It is plain (by Prop. LXX) that



the concentric sphærical superficies, of which the difference AEBF of the spheres is composed, have no effect at all upon the body P, their attractions being destroyed by contrary attractions. There remains, therefore, only the attraction of the interior sphere PEQF. And (by Prop. LXXII) this is as the distance PS. Q.E.D.

SCHOLIUM

By the superficies of which I here imagine the solids composed, I do not mean superficies purely mathematical, but orbs so extremely thin that their thickness is as nothing; that is, the evanescent orbs of which the sphere will at last consist, when the number of the orbs is increased, and their thickness diminished without end. In like manner, by the points of which lines, surfaces, and solids are said to be composed, are to be understood equal particles, whose magnitude is perfectly inconsiderable.

PROPOSITION LXXIV. THEOREM XXXIV.

The same things supposed, I say, that a corpuscle situate without the sphere is attracted with a force reciprocally proportional to the square of its distance from the centre.

For suppose the sphere to be divided into innumerable concentric sphærical superficies, and the attractions of the corpuscle arising from the several superficies will be reciprocally proportional to the square of the distance of the corpuscle from the centre of the sphere (by Prop. LXXI). And, by composition, the sum of those attractions, that is, the attraction of the corpuscle towards the entire sphere, will be in the same ratio. Q.E.D.

Cor. 1. Hence the attractions of homogeneous spheres at equal distances from the centres will be as the spheres themselves. For (by Prop. LXXII) if the distances be proportional to the diameters of the spheres, the forces will be as the diameters. Let the greater distance be diminished in that ratio; and the distances now being equal, the attraction will be increased in the duplicate of that ratio; and therefore will be to the other attraction in the triplicate of that ratio; that is, in the ratio of the spheres.

Cor. 2. At any distances whatever the attractions are as the spheres applied to the squares of the distances.

Cor. 3. If a corpuscle placed without an homogeneous sphere is attracted by a force reciprocally proportional to the square of its distance from the centre, and the sphere consists of attractive particles, the force of every particle will decrease in a duplicate ratio of the distance from each particle.

PROPOSITION LXXV. THEOREM XXXV.

If to the several points of a given sphere there tend equal centripetal forces decreasing in a duplicate ratio of the distances from the point; I say, that another similar sphere will be attracted by it with a force reciprocally proportional to the square of the distance of the centres.

For the attraction of every particle is reciprocally as the square of its distance from the centre of the attracting sphere (by Prop. LXXIV), and is therefore the same as if that whole attracting force issued from one single corpuscle placed in the centre of this sphere. But this attraction is as great as on the other hand the attraction of the same corpuscle would be if that were itself attracted by the several particles of the attracted sphere with the same force with which they are attracted by it. But that attraction of the corpuscle would be (by Prop. LXXIV) reciprocally proportional to the square of its distance from the centre of the sphere; therefore the attraction of the sphere, equal thereto, is also in the same ratio. Q.E.D.

Cor. 1. The attractions of spheres towards other homogeneous spheres are as the attracting spheres applied to the squares of the distances of their centres from the centres of those which they attract.

Cor. 2. The case is the same when the attracted sphere does also attract. For the several points of the one attract the several points of the other with the same force with which they themselves are attracted by the others again; and therefore since in all attractions (by Law III) the attracted and attracting point are both equally acted on, the force will be doubled by their mutual attractions, the proportions remaining.

Cor. 3. Those several truths demonstrated above concerning the motion of bodies about the focus of the conic sections will take place when an attracting sphere is placed in the focus, and the bodies move without the sphere.

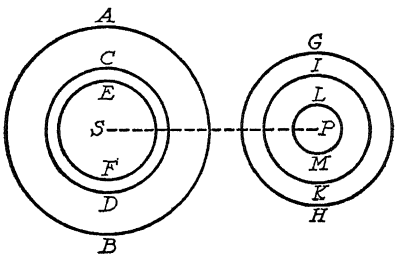
Cor. 4. Those things which were demonstrated before of the motion of bodies about the centre of the conic sections take place when the motions are performed within the sphere.

PROPOSITION LXXVI. THEOREM XXXVI.

If spheres be however dissimilar (as to density of matter and attractive force) in the same ratio onward from the centre to the circumference; but every where similar, at every given distance from the centre, on all sides round about; and the attractive force of every point decreases in the duplicate ratio of the distance of the body attracted; I say, that the whole force with which one of these spheres attracts the other will be reciprocally proportional to the square of the distance of the centres.

Imagine several concentric similar spheres, AB, CD, EF, &c., the innermost of which added to the outermost may compose a matter more dense towards the centre, or subducted from them may leave the same more lax and rare. Then, by Prop. LXXV, these spheres will attract other similar concentric spheres GH, IK, LM, &c., each the other, with forces reciprocally proportional to the square of the distance SP. And, by composition or division, the sum of all those forces, or the excess of any of

them above the others; that is, the entire force with which the whole sphere AB (composed of any concentric spheres or of their differences) will attract the whole sphere GH (composed of any concentric spheres or their differences) in the same ratio. Let the number of the concentric spheres be increased *in infinitum*, so that the density of the matter together with the attractive force may, in the progress from the circum-



ference to the centre, increase or decrease according to any given law; and by the addition of matter not attractive, let the deficient density be supplied, that so the spheres may acquire any form desired; and the force with which one of these attracts the other will be still, by the former reasoning, in the same ratio of the square of the distance inversely. Q.E.D.

Cor. 1. Hence if many spheres of this kind, similar in all respects, attract each other mutually, the accelerative attractions of each to each, at any equal distances of the centres, will be as the attracting spheres.

Cor. 2. And at any unequal distances, as the attracting spheres applied to the squares of the distances between the centres.

Cor. 3. The motive attractions, or the weights of the spheres towards one another, will be at equal distances of the centres as the attracting and attracted spheres conjunctly; that is, as the products arising from multiplying the spheres into each other.

Cor. 4. And at unequal distances, as those products directly, and the squares of the distances between the centres inversely.

Cor. 5. These proportions take place also when the attraction arises from the attractive virtue of both spheres mutually exerted upon each other. For the attraction is only doubled by the conjunction of the forces, the proportions remaining as before.

Cor. 6. If spheres of this kind revolve about others at rest, each about each; and the distances between the centres of the quiescent and revolving bodies are proportional to the diameters of the quiescent bodies; the periodic times will be equal.

Cor 7. And, again, if the periodic times are equal, the distances will be proportional to the diameters.

Cor. 8. All those truths above demonstrated, relating to the motions of bodies about the foci of conic sections, will take place when an attracting sphere, of any form and condition like that above described, is placed in the focus.

Cor. 9. And also when the revolving bodies are also attracting spheres of any condition like that above described.

PROPOSITION LXXVII. THEOREM XXXVII.

If to the several points of spheres there tend centripetal forces proportional to the distances of the points from the attracted bodies; I say, that the compounded force with which two spheres attract each other mutually is as the distance between the centres of the spheres.

PROPOSITION LXXVIII. THEOREM XXXVIII.

If spheres in the progress from the centre to the circumference be however dissimilar and unequable, but similar on every side round about at all given distances from the centre; and the attractive force of every point be as the distance of the attracted body; I say, that the entire force with which two spheres of this kind attract each other mutually is proportional to the distance between the centres of the spheres.

SCHOLIUM

I have now explained the two principal cases of attractions; to wit, when the centripetal forces decrease in a duplicate ratio of the distances, or increase in a simple ratio of the distances, causing the bodies in both cases to revolve in conic sections, and composing sphærical bodies whose centripetal forces observe the same law of increase or decrease in the recess from the centre as the forces or the particles themselves do; which is very remarkable. It would be tedious to run over the other cases, whose conclusions are less elegant and important, so particularly as I have done these.

Book Two: Of the Motion of Bodies

SECTION SIX

Of the motion and resistance of funependulous bodies

PROPOSITION XXIV. THEOREM XIX.

The quantities of matter in funependulous bodies, whose centres of oscillation are equally distant from the centre of suspension, are in a ratio compounded of the ratio of the weights and the duplicate ratio of the times of the oscillations in vacuo.

For the velocity which a given force can generate in a given matter in a given time is as the force and the time directly, and the matter inversely. The greater the force or the time is, or the less the matter, the greater velocity will be generated. This is manifest from the second Law of Motion. Now if pendulums are of the same length, the motive forces in places equally distant from the perpendicular are as the weights: and therefore if two bodies by oscillating describe equal arcs, and those arcs are divided into equal parts; since the times in which the bodies describe each of the correspondent parts of the arcs are as the times of the whole oscillations, the velocities in the correspondent parts of the oscillations will be to each other as the motive forces and the whole times of the oscillations directly, and the quantities of matter reciprocally: and therefore the quantities of matter are as the forces and the times of the oscillations directly and the velocities reciprocally. But the velocities reciprocally are as the times, and therefore the times directly and the velocities reciprocally are as the squares of the times; and therefore the quantities of matter are as the motive forces and the squares of the times, that is, as the weights and the squares of the times. Q.E.D.

Cor. 1. Therefore if the times are equal, the quantities of matter in each of the bodies are as the weights.

Cor. 2. If the weights are equal, the quantities of matter will be as the squares of the times.

Cor. 3. If the quantities of matter are equal, the weights will be reciprocally as the squares of the times.

Cor. 4. Whence since the squares of the times, *cæteris paribus,* are as the lengths of the pendulums, therefore if both the times and quantities of matter are equal, the weights will be as the lengths of the pendulums.

Cor. 5. And universally, the quantity of matter in the pendulous body is as the square of the time directly, and the length of the pendulum inversely.

Cor. 6. But in a non-resisting medium, the quantity of matter in the pendulous body is as the comparative weight and the square of the time directly, and the length of the pendulum inversely. For the comparative weight is the motive force of the body in any heavy medium, as was shown above; and therefore does the same thing in such a non-resisting medium as the absolute weight does in a vacuum.

Cor. 7. And hence appears a method both of comparing bodies one among another, as to the quantity of matter in each; and of comparing the weights of the same body in different places, to know the variation of its gravity. And by experiments made with the greatest accuracy, I have always found the quantity of matter in bodies to be proportional to their weight.

Book Three: Natural Philosophy

In the preceding Books I have laid down the principles of philosophy, principles not philosophical, but mathematical: such, to wit, as we may build our reasonings upon in philosophical inquiries. These principles are the laws and conditions of certain motions, and powers or forces, which chiefly have respect to philosophy; but, lest they should have appeared of themselves dry and barren, I have illustrated them here and there with some philosophical scholiums, giving an account of such things as are of more general nature, and which philosophy seems chiefly to be founded on; such as the density and the resistance of bodies, spaces void of all bodies, and the motion of light and sounds. It remains that, from the same principles, I now demonstrate the frame of the System of the World. Upon this subject I had, indeed, composed the third Book in a popular method, that it might be read by many; but afterward, considering that such as had not sufficiently entered into the principles could not easily discern the strength of the consequences, nor lay aside the prejudices to which they had been many years accustomed, therefore, to prevent the disputes which might be raised upon such accounts, I chose to reduce the substance of this Book into the form of Propositions (in the mathematical way), which should be read by those only who had first made themselves masters of the principles established in the preceding Books.

RULES OF REASONING IN PHILOSOPHY

RULE I

We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.

To this purpose the philosophers say that Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes.

RULE II

Therefore to the same natural effects we must, as far as possible, assign the same causes.

As to respiration in a man and in a beast; the descent of stones in *Europe* and in *America;* the light of our culinary fire and of the sun; the reflection of light in the earth, and in the planets.

RULE III

The qualities of bodies, which admit neither intension nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever.

For since the qualities of bodies are only known to us by experiments, we are to hold for universal all such as universally agree with experiments; and such as are not liable to diminution can never be quite taken away. We are certainly not to relinquish the evidence of experiments for the sake of dreams and vain fictions of our own devising; nor are we to recede from the analogy of Nature, which uses to be simple, and always consonant to itself. We no other way know the extension of bodies than by our senses, nor do these reach it in all bodies; but because we perceive extension in all that are sensible, therefore we ascribe it universally to all others also. That abundance of bodies are hard, we learn by experience; and because the hardness of the whole arises from the hardness of the parts, we therefore justly infer the hardness of the undivided particles not only of the bodies we feel but of all others. That all bodies are impenetrable, we gather not from reason, but from sensation. The bodies which we handle we find impenetrable, and thence conclude impenetrability to be an universal property of all bodies whatsoever. That all bodies are movable, and endowed with certain powers (which we call the *vires inertiæ*) of persevering in their motion, or in their rest, we only infer from the like properties observed in the bodies which we have seen. The extension, hardness, impenetrability, mobility, and *vis inertiæ* of the whole, result from the extension, hardness, impenetrability, mobility, and *vires inertiæ* of the parts; and thence we conclude the least particles of all bodies to be also all extended, and hard and impenetrable, and movable, and endowed with their proper *vires inertiæ*. And this is the foundation of all philosophy. Moreover, that the divided but contiguous particles of bodies may be separated from one another is matter of observation; and, in the particles that remain undivided, our minds are able to distinguish yet lesser parts, as is mathematically demonstrated. But whether the parts so distinguished, and not yet divided, may, by the powers of Nature, be actually

divided and separated from one another, we cannot certainly determine. Yet, had we the proof of but one experiment that any undivided particle, in breaking a hard and solid body, suffered a division, we might by virtue of this rule conclude that the undivided as well as the divided particles may be divided and actually separated to infinity.

Lastly, if it universally appears, by experiments and astronomical observations, that all bodies about the earth gravitate towards the earth, and that in proportion to the quantity of matter which they severally contain; that the moon likewise, according to the quantity of its matter, gravitates towards the earth; that, on the other hand, our sea gravitates towards the moon; and all the planets mutually one towards another; and the comets in like manner towards the sun; we must, in consequence of this rule, universally allow that all bodies whatsoever are endowed with a principle of mutual gravitation. For the argument from the appearances concludes with more force for the universal gravitation of all bodies than for their impenetrability; of which, among those in the celestial regions, we have no experiments, nor any manner of observation. Not that I affirm gravity to be essential to bodies: by their *vis insita* I mean nothing but their *vis inertiæ*. This is immutable. Their gravity is diminished as they recede from the earth.

In experimental philosophy we are to look upon propositions collected by general induction from phænomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phænomena occur, by which they may either be made more accurate or liable to exceptions.

This rule we must follow, that the argument of induction may not be evaded by hypotheses.

PHÆNOMENA, OR APPEARANCES

PHÆNOMENON I

That the circumjovial planets, by radii drawn to Jupiter's centre, describe areas proportional to the times of description; and that their periodic times, the fixed stars being at rest, are in the sesquiplicate proportion of their distances from its centre.

This we know from astronomical observations. For the orbits of these planets differ but insensibly from circles concentric to Jupiter; and their motions in those circles are found to be uniform. And all astronomers agree that their periodic times are in the sesquiplicate proportion of the semi-diameters of their orbits; and so it manifestly appears from the following table.

The periodic times of the satellites of Jupiter.

1ᵈ. 18ʰ. 27′ 34″. 3ᵈ. 13ʰ. 13′ 42″. 7ᵈ. 3ʰ. 42′ 36″. 16ᵈ. 16ʰ. 32′ 9″.

The distances of the satellites from Jupiter's centre.

From the observations of	1	2	3	4	
Borelli	5⅔	8⅔	14	24⅔	*semi-diameter of Jupiter*
Townly *by the Microm.* . .	5,52	8,78	13,47	24,72	
Cassini *by the Telescope* . .	5	8	13	23	
Cassini *by the eclip. of the satel.*	5⅔	9	14 23/60	25 3/10	
From the periodic times	5,667	9,017	14,384	25,299	

PHÆNOMENON II

That the circumsaturnal planets, by radii drawn to Saturn's centre, describe areas proportional to the times of description; and that their periodic times, the fixed stars being at rest, are in the sesquiplicate proportion of their distances from its centre.

PHÆNOMENON III

That the five primary planets, Mercury, Venus, Mars, Jupiter, and Saturn, with their several orbits, encompass the sun.

That Mercury and Venus revolve about the sun is evident from their moon-like appearances. When they shine out with a full face, they are, in respect of us, beyond or above the sun; when they appear half full, they are about the same height on one side or other of the sun; when horned, they are below or between us and the sun; and they are sometimes, *when directly under,* seen like spots traversing the sun's disk. That Mars surrounds the sun is as plain from its full face when near its conjunction with the sun, and from the gibbous figure which it shews in its quadratures. And the same thing is demonstrable of Jupiter and Saturn, from their appearing full in all situations; for the shadows of their satellites that appear sometimes upon their disks make it plain that the light they shine with is not their own, but borrowed from the sun.

PHÆNOMENON IV

That the fixed stars being at rest, the periodic times of the five primary planets, and (whether of the sun about the earth, or) of the earth about the sun, are in the sesquiplicate proportion of their mean distances from the sun.

This proportion, first observed by *Kepler,* is now received by all astronomers; for the periodic times are the same, and the dimensions of the

orbits are the same, whether the sun revolves about the earth, or the earth about the sun. And as to the measures of the periodic times, all astronomers are agreed about them.

PHÆNOMENON V

Then the primary planets, by radii drawn to the earth, describe areas no wise proportional to the times; but that the areas which they describe by radii drawn to the sun are proportional to the times of description.

For to the earth they appear sometimes direct, sometimes stationary, nay, and sometimes retrograde. But from the sun they are always seen direct, and to proceed with a motion nearly uniform, that is to say, a little swifter in the perihelion and a little slower in the aphelion distances, so as to maintain an equality in the description of the areas. This a noted proposition among astronomers, and particularly demonstrable in Jupiter, from the eclipses of his satellites; by the help of which eclipses, as we have said, the heliocentric longitudes of that planet, and its distances from the sun, are determined.

PHÆNOMENON VI

That the moon, by a radius drawn to the earth's centre, describes an area proportional to the time of description.

This we gather from the apparent motion of the moon, compared with its apparent diameter. It is true that the motion of the moon is a little disturbed by the action of the sun: but in laying down these Phænomena, I neglect those small and inconsiderable errors.

PROPOSITIONS

PROPOSITION I. THEOREM I.

That the forces by which the circumjovial planets are continually drawn off from rectilinear motions, and retained in their proper orbits, tend to Jupiter's centre; and are reciprocally as the squares of the distances of the places of those planets *from that centre.*

The former part of this Proposition appears from Phæn. I and Prop. II or III, Book One; the latter from Phæn. I and Cor. 6, Prop. IV, of the same Book.

The same thing we are to understand of the planets which encompass Saturn, by Phæn. II.

PROPOSITION II. THEOREM II.

That the forces by which the primary planets are continually drawn off from rectilinear motions, and retained in their proper orbits, tend to the sun; and are reciprocally as the squares of the distances of the places of those planets *from the sun's centre.*

The former part of the Proposition is manifest from Phæn. V and Prop. II, Book One; the latter from Phæn. IV and Cor. 6, Prop. IV, of the same Book. But this part of the Proposition is, with great accuracy, demonstrable from the quiescence of the aphelion points; for a very small aberration from the *reciprocal* duplicate proportion would produce a motion of the apsides sensible enough in every single revolution, and in many of them enormously great.

PROPOSITION III. THEOREM III.

That the force by which the moon is retained in its orbit tends to the earth; and is reciprocally as the square of the distance of its place from the earth's centre.

The former part of the Proposition is evident from Phæn. VI and Prop. II or III, Book One; the latter from the very slow motion of the moon's apogee; which in every single revolution amounting but to 3° 3′ *in consequentia,* may be neglected.

PROPOSITION IV. THEOREM IV.

That the moon gravitates towards the earth, and by the force of gravity is continually drawn off from a rectilinear motion, and retained in its orbit.

The mean distance of the moon from the earth in the syzygies in semi-diameters of the earth is, according to *Ptolemy* and most astronomers, 59; according to *Vendelin* and *Huygens,* 60; to *Copernicus,* 60⅓; to *Street,* 60⅖; and to *Tycho,* 56½. But *Tycho,* and all that follow his tables of refraction, making the refractions of the sun and moon (altogether against the nature of light) to exceed the refractions of the fixed stars, and that by four or five minutes *near the horizon,* did thereby increase the moon's *horizontal* parallax by a like number of minutes, that is, by a twelfth or fifteenth part of the whole parallax. Correct this error, and the distance will become about 60½ semi-diameters of the earth, near to what others have assigned. Let us assume the mean distance of 60 diameters in the syzygies; and suppose one revolution of the moon, in respect of the fixed stars, to be completed in $27^{d}.\ 7^{h}.\ 43'$, as astronomers have de-

termined; and the circumference of the earth to amount to 123249600 *Paris* feet, as the French have found by mensuration. And now if we imagine the moon, deprived of all motion, to be let go, so as to descend towards the earth with the impulse of all that force by which (by Prop. III) it is retained in its orb, it will in the space of one minute of time, describe in its fall $15\frac{1}{12}$ *Paris* feet. This we gather by a calculus, founded upon Cor. 9, Prop. IV, of the same Book. For the versed sine of that arc, which the moon, in the space of one minute of time, would by its mean motion describe at the distance of 60 semi-diameters of the earth, is nearly $15\frac{1}{12}$ *Paris* feet, or more accurately 15 feet, 1 inch, and 1 line $\frac{4}{9}$. Wherefore, since that force, in approaching to the earth, increases in the reciprocal duplicate proportion of the distance, and, upon that account, at the surface of the earth, is 60×60 times greater than at the moon, a body in our regions, falling with that force, ought, in the space of one minute of time, to describe $60\times60\times15\frac{1}{12}$ *Paris* feet; and, in the space of one second of time, to describe $15\frac{1}{12}$ of those feet; or, more accurately, 15 feet, 1 inch, and 1 line $\frac{4}{9}$. And with this very force we actually find that bodies here upon earth do really descend; for a pendulum oscillating seconds in the latitude of *Paris* will be 3 *Paris* feet, and 8 lines $\frac{1}{2}$ in length, as Mr. *Huygens* has observed. And the space which a heavy body describes by falling in one second of time is to half the length of this pendulum in the duplicate ratio of the circumference of a circle to its diameter (as Mr. *Huygens* has also shewn), and is therefore 15 *Paris* feet, 1 inch, 1 line $\frac{7}{9}$. And therefore the force by which the moon is retained in its orbit becomes, at the very surface of the earth, equal to the force of gravity which we observe in heavy bodies there. And therefore (by Rule I and II) the force by which the moon is retained in its orbit is that very same force which we commonly call gravity; for, were gravity another force different from that, then bodies descending to the earth with the joint impulse of both forces would fall with a double velocity, and in the space of one second of time would describe $30\frac{1}{6}$ *Paris* feet; altogether against experience.

This calculus is founded on the hypothesis of the earth's standing still; for if both earth and moon move about the sun, and at the same time about their common centre of gravity, the distance of the centres of the moon and earth from one another will be $60\frac{1}{2}$ semi-diameters of the earth.

SCHOLIUM

The demonstration of this Proposition may be more diffusely explained after the following manner. Suppose several moons to revolve about the earth, as in the system of Jupiter or Saturn; the periodic times of these moons (by the argument of induction) would observe the same law which *Kepler* found to obtain among the planets; and therefore their centripetal forces would be reciprocally as the squares of the distances from the centre of the earth, by Prop. I of this Book. Now if the lowest of

these were very small, and were so near the earth as almost to touch the tops of the highest mountains, the centripetal force thereof, retaining it in its orb, would be very nearly equal to the weights of any *terrestrial* bodies that should be found upon the tops of those mountains, as may be known by the foregoing computation. Therefore if the same little moon should be deserted by its centrifugal force that carries it through its orb, and so be disabled from going onward therein, it would descend to the earth; and that with the same velocity as heavy bodies do actually fall with upon the tops of those very mountains; because of the equality of the forces that oblige them both to descend. And if the force by which that lowest moon would descend were different from gravity, and if that moon were to gravitate towards the earth, as we find terrestrial bodies do upon the tops of mountains, it would then descend with twice the velocity, as being impelled by both these forces conspiring together. Therefore since both these forces, that is, the gravity of heavy bodies, and the centripetal forces of the moons, respect the centre of the earth, and are similar and equal between themselves, they will (by Rule I and II) have one and the same cause. And therefore the force which retains the moon in its orbit is that very force which we commonly call gravity; because otherwise this little moon at the top of a mountain must either be without gravity or fall twice as swiftly as heavy bodies are wont to do.

PROPOSITION V. THEOREM V.

That the circumjovial planets gravitate towards Jupiter; the circumsaturnal towards Saturn; the circumsolar towards the sun; and by the forces of their gravity are drawn off from rectilinear motions, and retained in curvilinear orbits.

Cor. 1. There is, therefore, a power of gravity tending to all the planets; for, doubtless, Venus, Mercury, and the rest, are bodies of the same sort with Jupiter and Saturn. And since all attraction (by Law III) is mutual, Jupiter will therefore gravitate towards all his own satellites, Saturn towards his, the earth towards the moon, and the sun towards all the primary planets.

Cor. 2. The force of gravity which tends to any one planet is reciprocally as the square of the distance of places from that planet's centre.

Cor. 3. All the planets do mutually gravitate towards one another, by Cor. 1 and 2. And hence it is that Jupiter and Saturn, when near their conjunction, by their mutual attractions sensibly disturb each other's motions. So the sun disturbs the motions of the moon; and both sun and moon disturb our sea, as we shall hereafter explain.

SCHOLIUM

The force which retains the celestial bodies in their orbits has been hitherto called centripetal force; but it being now made plain that it can

be no other than a gravitating force, we shall hereafter call it gravity. For the cause of that centripetal force which retains the moon in its orbit will extend itself to all the planets, by Rules I, II, and IV.

PROPOSITION VI. THEOREM VI.

That all bodies gravitate towards every planet; and that the weights of bodies towards any the same *planet, at equal distances from the centre of the planet, are proportional to the quantities of matter which they severally contain.*

It has been, now of a long time, observed by others that all sorts of heavy bodies (allowance being made for the inequality of retardation which they suffer from a small power of resistance in the air) descend to the earth *from equal heights* in equal times; and that equality of times we may distinguish to a great accuracy, by the help of pendulums. I tried the thing in gold, silver, lead, glass, sand, common salt, wood, water, and wheat. I provided two wooden boxes, round and equal: I filled the one with wood, and suspended an equal weight of gold (as exactly as I could) in the centre of oscillation of the other. The boxes hanging by equal threads of 11 feet made a couple of pendulums perfectly equal in weight and figure, and equally receiving the resistance of the air. And, placing the one by the other, I observed them to play together forward and backward, for a long time, with equal vibrations. And therefore the quantity of matter in the gold (by Cor. 1 and 6, Prop. XXIV, Book Two) was to the quantity of matter in the wood as the action of the motive force (or *vis motrix*) upon all the gold to the action of the same upon all the wood; that is, as the weight of the one to the weight of the other: and the like happened in the other bodies. By these experiments, in bodies of the same weight, I could manifestly have discovered a difference of matter less than the thousandth part of the whole, had any such been. But, without all doubt, the nature of gravity towards the planets is the same as towards the earth. For, should we imagine our terrestrial bodies removed to the orb of the moon, and there, together with the moon, deprived of all motion, to be let go, so as to fall together towards the earth, it is certain, from what we have demonstrated before, that, in equal times, they would describe equal spaces with the moon, and of consequence are to the moon, in quantity of matter, as their weights to its weight. Moreover, since the satellites of Jupiter perform their revolutions in times which observe the sesquiplicate proportion of their distances from Jupiter's centre, their accelerative gravities towards Jupiter will be reciprocally as the squares of their distances from Jupiter's centre; that is, equal, at equal distances. And, therefore, these satellites, if supposed to fall *towards Jupiter* from equal heights, would describe equal spaces in equal times, in like manner as heavy bodies do on our earth. And, by the same argument, if the circumsolar planets were supposed to be let fall at equal distances from the sun, they would, in their descent towards the sun, describe equal spaces

in equal times. But forces which equally accelerate unequal bodies must be as those bodies: that is to say, the weights of the planets *towards the sun* must be as their quantities of matter. Further, that the weights of Jupiter and of his satellites towards the sun are proportional to the several quantities of their matter appears from the exceedingly regular motions of the satellites. For if some of those bodies were more strongly attracted to the sun in proportion to their quantity of matter than others, the motions of the satellites would be disturbed by that inequality of attraction. If, at equal distances from the sun, any satellite, in proportion to the quantity of its matter, did gravitate towards the sun with a force greater than Jupiter in proportion to his, according to any given proportion, suppose of *d* to *e;* then the distance between the centres of the sun and of the satellite's orbit would be always greater than the distance between the centres of the sun and of Jupiter nearly in the subduplicate of that proportion: as by some computations I have found. And if the satellite did gravitate towards the sun with a force, lesser in the proportion of *e* to *d,* the distance of the centre of the satellite's orb from the sun would be less than the distance of the centre of Jupiter from the sun in the subduplicate of the same proportion. Therefore if, at equal distances from the sun, the accelerative gravity of any satellite towards the sun were greater or less than the accelerative gravity of Jupiter towards the sun but by one $\frac{1}{1000}$ part of the whole gravity, the distance of the centre of the satellite's orbit from the sun would be greater or less than the distance of Jupiter from the sun by one $\frac{1}{2000}$ part of the whole distance; that is, by a fifth part of the distance of the utmost satellite from the centre of Jupiter; an eccentricity of the orbit which would be very sensible. But the orbits of the satellites are concentric to Jupiter, and therefore the accelerative gravities of Jupiter, and of all its satellites towards the sun, are equal among themselves. And by the same argument, the weights of Saturn and of his satellites towards the sun, at equal distances from the sun, are as their several quantities of matter; and the weights of the moon and of the earth towards the sun are either none, or accurately proportional to the masses of matter which they contain. But some they are, by Cor. 1 and 3, Prop. V.

But further; the weights of all the parts of every planet towards any other planet are one to another as the matter in the several parts; for if some parts did gravitate more, others less, than for the quantity of their matter, then the whole planet, according to the sort of parts with which it most abounds, would gravitate more or less than in proportion to the quantity of matter in the whole. Nor is it of any moment whether these parts are external or internal; for if, for example, we should imagine the terrestrial bodies with us to be raised up to the orb of the moon, to be there compared with its body: if the weights of such bodies were to the weights of the external parts of the moon as the quantities of matter in the one and in the other respectively; but to the weights of the internal parts in a greater or less proportion, then likewise the weights of those bodies would be to the weight of the whole moon in a greater or less proportion; against what we have shewed above.

Cor. 1. Hence the weights of bodies do not depend upon their forms and textures; for if the weights could be altered with the forms, they would be greater or less, according to the variety of forms, in equal matter; altogether against experience.

Cor. 2. Universally, all bodies about the earth gravitate towards the earth; and the weights of all, at equal distances from the earth's centre, are as the quantities of matter which they severally contain. This is the quality of all bodies within the reach of our experiments; and therefore (by Rule III) to be affirmed of all bodies whatsoever.

Cor. 3. All spaces are not equally full; for if all spaces were equally full, then the specific gravity of the fluid which fills the region of the air, on account of the extreme density of the matter, would fall nothing short of the specific gravity of quicksilver, or gold, or any other the most dense body; and, therefore, neither gold, nor any other body, could descend in air; for bodies do not descend in fluids, unless they are specifically heavier than the fluids. And if the quantity of matter in a given space can, by any rarefaction, be diminished, what should hinder a diminution to infinity?

Cor. 4. If all the solid particles of all bodies are of the same density, nor can be rarefied without pores, a void, space, or vacuum must be granted. By bodies of the same density, I mean those whose *vires inertiæ* are in the proportion of their bulks.

Cor. 5. The power of gravity is of a different nature from the power of magnetism; for the magnetic attraction is not as the matter attracted. Some bodies are attracted more by the magnet; others less; most bodies not at all. The power of magnetism in one and the same body may be increased and diminished; and is sometimes far stronger, for the quantity of matter, than the power of gravity; and in receding from the magnet decreases not in the duplicate but almost in the triplicate proportion of the distance, as nearly as I could judge from some rude observations.

PROPOSITION VII. THEOREM VII.

That there is a power of gravity tending to all bodies, proportional to the several quantities of matter which they contain.

That all the planets mutually gravitate one towards another, we have proved before; as well as that the force of gravity towards every one of them, considered apart, is reciprocally as the square of the distance of places from the centre of the planet. And thence it follows that the gravity tending towards all the planets is proportional to the matter which they contain.

Moreover, since all the parts of any planet A gravitate towards any other planet B; and the gravity of every part is to the gravity of the whole as the matter of the part to the matter of the whole; and (by Law III) to every action corresponds an equal re-action; therefore the planet B will, on the other hand, gravitate towards all the parts of the planet A;

and its gravity towards any one part will be to the gravity towards the whole as the matter of the part to the matter of the whole. Q.E.D.

Cor. 1. Therefore the force of gravity towards any whole planet arises from, and is compounded of, the forces of gravity towards all its parts. Magnetic and electric attractions afford us examples of this; for all attraction towards the whole arises from the attractions towards the several parts. The thing may be easily understood in gravity, if we consider a greater planet, as formed of a number of lesser planets, meeting together in one globe; for *hence it would appear that* the force of the whole must arise from the forces of the component parts. If it is objected, that, according to this law, all bodies with us must mutually gravitate one towards another, whereas no such gravitation any where appears, I answer, that since the gravitation towards these bodies is to the gravitation towards the whole earth as these bodies are to the whole earth, the gravitation towards them must be far less than to fall under the observation of our senses.

Cor. 2. The force of gravity towards the several equal particles of any body is reciprocally as the square of the distance of places from the particles; as appears from Cor. 3, Prop. LXXIV, Book One.

PROPOSITION VIII. THEOREM VIII.

In two spheres mutually gravitating each towards the other, if the matter in places on all sides round about and equidistant from the centres is similar, the weight of either sphere towards the other will be reciprocally as the square of the distance between their centres.

After I had found that the force of gravity towards a whole planet did arise from and was compounded of the forces of gravity towards all its parts, and towards every one part was in the reciprocal proportion of the squares of the distances from the part, I was yet in doubt whether that reciprocal duplicate proportion did accurately hold, or but nearly so, in the total force compounded of so many partial ones; for it might be that the proportion which accurately enough took place in greater distances should be wide of the truth near the surface of the planet, where the distances of the particles are unequal, and their situation dissimilar. But by the help of Prop. LXXV and LXXVI, Book One, and their Corollaries, I was at last satisfied of the truth of the Proposition, as it now lies before us.

Cor. 1. Hence we may find and compare together the weights of bodies towards different planets; for the weights of bodies revolving in circles about planets are (by Cor. 2, Prop. IV, Book One) as the diameters of the circles directly, and the squares of their periodic times reciprocally; and their weights at the surfaces of the planets, or at any other distances from their centres, are (by this Prop.) greater or less in the reciprocal duplicate proportion of the distances. Thus from the periodic times of Venus, revolving about the sun, in $224^{d}.\ 16\frac{3}{4}^{h}$, of the utmost circumjovial satellite revolving about Jupiter, in $16^{d}.\ 16\frac{8}{15}^{h}$.; of the Huygenian satel-

lite about Saturn in 15^{d}. $22\frac{2}{3}^{h}$.; and of the moon about the earth in 27^{d}. 7^{h}. 43′; compared with the mean distance of Venus from the sun, and with the greatest heliocentric elongations of the outmost circumjovial satellite from Jupiter's centre, 8′ 16″; of the Huygenian satellite from the centre of Saturn, 3′ 4″; and of the moon from the earth, 10′ 33″: by computation I found that the weight of equal bodies, at equal distances from the centres of the sun, of Jupiter, of Saturn, and of the earth, towards the sun, Jupiter, Saturn, and the earth, were one to another, as 1, $\frac{1}{1067}$, $\frac{1}{3021}$, and $\frac{1}{169282}$ respectively. Then because as the distances are increased or diminished, the weights are diminished or increased in a duplicate ratio, the weights of equal bodies towards the sun, Jupiter, Saturn, and the earth, at the distances 10000, 997, 791, and 109 from their centres, that is, at their very superficies, will be as 10000, 943, 529, and 435 respectively. How much the weights of bodies are at the superficies of the moon will be shewn hereafter.

Cor. 2. Hence likewise we discover the quantity of matter in the several planets; for their quantities of matter are as the forces of gravity at equal distances from their centres; that is, in the sun, Jupiter, Saturn, and the earth, as 1, $\frac{1}{1067}$, $\frac{1}{3021}$, and $\frac{1}{169282}$ respectively. If the parallax of the sun be taken greater or less than 10″ 30‴, the quantity of matter in the earth must be augmented or diminished in the triplicate of that proportion.

Cor. 3. Hence also we find the densities of the planets; for (by Prop. LXXII, Book One) the weights of equal and similar bodies towards similar spheres are, at the surfaces of those spheres, as the diameters of the spheres; and therefore the densities of dissimilar spheres are as those weights applied to the diameters of the spheres. But the true diameters of the sun, Jupiter, Saturn, and the earth were one to another as 10000, 997, 791, and 109; and the weights towards the same as 10000, 943, 529, and 435 respectively; and therefore their densities are as 100, $94\frac{1}{2}$, 67, and 400. The density of the earth, which comes out by this computation, does not depend upon the parallax of the sun, but is determined by the parallax of the moon, and therefore is here truly defined. The sun, therefore, is a little denser than Jupiter, and Jupiter than Saturn, and the earth four times denser than the sun; for the sun, by its great heat, is kept in a sort of a rarefied state. The moon is denser than the earth, as shall appear afterward.

Cor. 4. The smaller the planets are, they are, *cæteris paribus,* of so much the greater density; for so the powers of gravity on their several surfaces come nearer to equality. They are likewise, *cæteris paribus,* of the greater density, as they are nearer to the sun. So Jupiter is more dense than Saturn, and the earth than Jupiter; for the planets were to be placed at different distances from the sun, that, according to their degrees of density, they might enjoy a greater or less proportion to the sun's heat. Our water, if it were removed as far as the orb of Saturn, would be converted into ice, and in the orb of Mercury would quickly fly away in vapour; for the light of the sun, to which its heat is proportional, is seven

times denser in the orb of Mercury than with us: and by the thermometer I have found that a sevenfold heat of our summer sun will make water boil. Nor are we to doubt that the matter of Mercury is adapted to its heat, and is therefore more dense than the matter of our earth; since, in a denser matter, the operations of Nature require a stronger heat.

PROPOSITION IX. THEOREM IX.

That the force of gravity, considered downward from the surface of the planets, decreases nearly in the proportion of the distances from their centres.

PROPOSITION X. THEOREM X.

That the motions of the planets in the heavens may subsist an exceedingly long time.

I have shewed that a globe of water frozen into ice, and moving freely in our air, in the time that it would describe the length of its semi-diameter, would lose by the resistance of the air $\frac{1}{4586}$ part of its motion; and the same proportion holds nearly in all globes, how great soever, and moved with whatever velocity. But that our globe of earth is of greater density than it would be if the whole consisted of water only, I thus make out. If the whole consisted of water only, whatever was of less density than water, because of its less specific gravity, would emerge and float above. And upon this account, if a globe of terrestrial matter, covered on all sides with water, was less dense than water, it would emerge somewhere; and, the subsiding water falling back, would be gathered to the opposite side. And such is the condition of our earth, which in a great measure is covered with seas. The earth, if it was not for its greater density, would emerge from the seas, and, according to its degree of levity, would be raised more or less above their surface, the water of the seas flowing backward to the opposite side. By the same argument, the spots of the sun, which float upon the lucid matter thereof, are lighter than that matter; and, however the planets have been formed while they were yet in fluid masses, all the heavier matter subsided to the centre. Since, therefore, the common matter of our earth on the surface thereof is about twice as heavy as water, and a little lower, in mines, is found about three, or four, or even five times more heavy, it is probable that the quantity of the whole matter of the earth may be five or six times greater than if it consisted all of water; especially since I have before shewed that the earth is about four times more dense than Jupiter. If, therefore, Jupiter is a little more dense than water, in the space of thirty days, in which that planet describes the length of 459 of its semi-diameters, it would, in a medium of the same density with our air, lose almost a tenth part of its motion. But since the resistance of mediums decreases in proportion to their weight or density, so that water, which is $13\frac{3}{5}$ times lighter than quick-

silver, resists less in that proportion; and air, which is 860 times lighter than water, resists less in the same proportion; therefore in the heavens, where the weight of the medium in which the planets move is immensely diminished, the resistance will almost vanish.

It is shewn that at the height of 200 miles above the earth the air is more rare than it is at the superficies of the earth in the ratio of 30 to 0,00000000000003998, or as 75000000000000 to 1 nearly. And hence the planet Jupiter, revolving in a medium of the same density with that superior air, would not lose by the resistance of the medium the 10000000th part of its motion in 1000000 years. In the spaces near the earth the resistance is produced only by the air, exhalations, and vapours. When these are carefully exhausted by the air pump from under the receiver, heavy bodies fall within the receiver with perfect freedom, and without the least sensible resistance: gold itself, and the lightest down, let fall together, will descend with equal velocity; and though they fall through a space of four, six, and eight feet, they will come to the bottom at the same time; as appears from experiments. And therefore the celestial regions being perfectly void of air and exhalations, the planets and comets meeting no sensible resistance in those spaces will continue their motions through them for an immense tract of time.

HYPOTHESIS I

That the centre of the system of the world is immovable.

This is acknowledged by all, while some contend that the earth, others that the sun, is fixed in that center. Let us see what may from hence follow.

PROPOSITION XI. THEOREM XI.

That the common centre of gravity of the earth, the sun, and all the planets is immovable.

For (by Cor. 4 of the Laws) that centre either is at rest or moves uniformly forward in a right line; but if that centre moved, the centre of the world would move also, against the Hypothesis.

PROPOSITION XII. THEOREM XII.

That the sun is agitated by a perpetual motion, but never recedes far from the common centre of gravity of all the planets.

For since (by Cor. 2, Prop. VIII) the quantity of matter in the sun is to the quantity of matter in Jupiter as 1067 to 1; and the distance of Jupiter from the sun is to the semi-diameter of the sun in a proportion but a small matter greater, the common centre of gravity of Jupiter and the sun

will fall upon a point a little without the surface of the sun. By the same argument, since the quantity of matter in the sun is to the quantity of matter in Saturn as 3021 to 1, and the distance of Saturn from the sun is to the semi-diameter of the sun in a proportion but a small matter less, the common centre of gravity of Saturn and the sun will fall upon a point a little within the surface of the sun. And, pursuing the principles of this computation, we should find that though the earth and all the planets were placed on one side of the sun, the distance of the common centre of gravity of all from the centre of the sun would scarcely amount to one diameter of the sun. In other cases, the distances of those centres are always less; and therefore, since that centre of gravity is in perpetual rest, the sun, according to the various positions of the planets, must perpetually be moved every way, but will never recede far from that centre.

Cor. Hence the common centre of gravity of the earth, the sun, and all the planets is to be esteemed the centre of the world; for since the earth, the sun, and all the planets mutually gravitate one towards another, and are therefore, according to their powers of gravity, in perpetual agitation, as the Laws of Motion require, it is plain that their movable centres cannot be taken for the immovable centre of the world. If that body were to be placed in the centre, towards which other bodies gravitate most (according to common opinion), that privilege ought to be allowed to the sun; but since the sun itself is moved, a fixed point is to be chosen from which the centre of the sun recedes least, and from which it would recede yet less if the body of the sun were denser and greater, and therefore less apt to be moved.

PROPOSITION XIII. THEOREM XIII.

The planets move in ellipses which have their common focus in the centre of the sun; and, by radii drawn to that centre, they describe areas proportional to the times of description.

We have discoursed above of these motions from the Phænomena. Now that we know the principles on which they depend, from those principles we deduce the motions of the heavens *a priori*. Because the weights of the planets towards the sun are reciprocally as the squares of their distances from the sun's centre, if the sun was at rest, and the other planets did not mutually act one upon another, their orbits would be ellipses, having the sun in their common focus; and they would describe areas proportional to the times *of description,* by Prop. I, Book One. But the mutual actions of the planets one upon another are so very small, that they may be neglected.

It is true that the action of Jupiter upon Saturn is not to be neglected: for the force of gravity towards Jupiter is to the force of gravity towards the sun as 1 to 1067; and therefore in the conjunction of Jupiter and Saturn, because the distance of Saturn from Jupiter is to the distance of Saturn from the sun almost as 4 to 9, the gravity of Saturn towards Jupi-

ter will be to the gravity of Saturn towards the sun as 81 to 16×1067; or, as 1 to about 211. And hence arises a perturbation of the orb of Saturn in every conjunction of this planet with Jupiter, so sensible that astronomers are puzzled with it. As the planet is differently situated in these conjunctions, its eccentricity is sometimes augmented, sometimes diminished; its aphelion is sometimes carried forward, sometimes backward, and its mean motion is by turns accelerated and retarded; yet the whole error in its motion about the sun, though arising from so great a force, may be almost avoided (except in the mean motion) by placing the lower focus of its orbit in the common centre of gravity of Jupiter and the sun, and therefore that error, when it is greatest, scarcely exceeds two minutes; and the greatest error in the mean motion scarcely exceeds two minutes yearly. But in the conjunction of Jupiter and Saturn, the accelerative forces of gravity of the sun towards Saturn, of Jupiter towards Saturn, and of Jupiter towards the sun, are almost as 16, 81, and $\frac{16\times81\times3021}{25}$, or 156609; and therefore the difference of the forces of gravity of the sun towards Saturn, and of Jupiter towards Saturn, is to the force of gravity of Jupiter towards the sun as 65 to 156609, or as 1 to 2409. But the greatest power of Saturn to disturb the motion of Jupiter is proportional to this difference; and therefore the perturbation of the orbit of Jupiter is much less than that of Saturn's. The perturbations of the other orbits are yet far less, except that the orbit of the earth is sensibly disturbed by the moon. The common centre of gravity of the earth and moon moves in an ellipsis about the sun in the focus thereof, and, by a radius drawn to the sun, describes areas proportional to the times of description. But the earth in the meantime by a menstrual motion is revolved about this common centre.

PROPOSITION XIV. THEOREM XIV.

The aphelions and nodes of the orbits of the planets are fixed.

It is true that some inequalities may arise from the mutual actions of the planets and comets in their revolutions; but these will be so small that they may be here passed by.

Cor. 1. The fixed stars are immovable, seeing they keep the same position to the aphelions and nodes of the planets.

Cor. 2. And since these stars are liable to no sensible parallax from the annual motion of the earth, they can have no force, because of their immense distance, to produce any sensible effect in our system. Not to mention that the fixed stars, every where promiscuously dispersed in the heavens, by their contrary attractions destroy their mutual actions, by Prop. LXX, Book One.

SCHOLIUM

Since the planets near the sun (viz. Mercury, Venus, the Earth, and Mars) are so small that they can act with but little force upon each other, therefore their aphelions and nodes must be fixed, excepting in so far as they are disturbed by the actions of Jupiter and Saturn, and other higher bodies. And hence we may find, by the theory of gravity, that their aphelions move a little *in consequentia,* in respect of the fixed stars, and that in the sesquiplicate proportion of their several distances from the sun. So that if the aphelion of Mars, in the space of a hundred years, is carried 33′ 20″ *in consequentia,* in respect of the fixed stars, the aphelions of the Earth, of Venus, and of Mercury, will in a hundred years be carried forwards 17′ 40″, 10′ 53″, and 4′ 16″, respectively. But these motions are so inconsiderable that we have neglected them in this Proposition.

PROPOSITION XV. PROBLEM I.

To find the principal diameters of the orbits of the planets.

They are to be taken in the sub-sesquiplicate proportion of the periodic times.

PROPOSITION XVI. PROBLEM II.

To find the eccentricities and aphelions of the planets.

PROPOSITION XVII. THEOREM XV.

That the diurnal motions of the planets are uniform, and that the libration of the moon arises from its diurnal motion.

PROPOSITION XVIII. THEOREM XVI.

That the axes of the planets are less than the diameters drawn perpendicular to the axes.

The equal gravitation of the parts on all sides would give a spherical figure to the planets, if it was not for their diurnal revolution in a circle. By that circular motion it comes to pass that the parts receding from the axis endeavour to ascend about the equator; and therefore if the matter is in a fluid state, by its ascent towards the equator it will enlarge the diameters there, and by its descent towards the poles it will shorten the axis. So the diameter of Jupiter (by the concurring observations of astrono-

mers) is found shorter betwixt pole and pole than from east to west. And, by the same argument, if our earth was not higher about the equator than at the poles, the seas would subside about the poles, and, rising towards the equator, would lay all things there under water.

PROPOSITION XIX. PROBLEM III.

To find the proportion of the axis of a planet to the diameters perpendicular thereto.

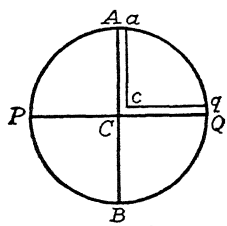
Our countryman, Mr. *Norwood,* measuring a distance of 995751 feet of *London* measure between *London* and *York,* in 1635, and observing the difference of latitudes to be 2° 28′, determined the measure of one degree to be 367196 feet of *London* measure, that is, 57300 *Paris* toises. M. *Picart,* measuring an arc of one degree, and 22′ 55″ of the meridian between *Amiens* and *Malvoisine,* found an arc of one degree to be 57060 *Paris* toises. M. *Cassini,* the father, measured the distance upon the meridian from the town of *Collioure* in *Roussillon* to the Observatory of *Paris;* and his son added the distance from the Observatory to the Citadel of *Dunkirk.* The whole distance was 486156½ toises and the difference of the latitudes of *Collioure* and *Dunkirk* was 8 degrees, and 31′ 11⅝″. Hence an arc of one degree appears to be 57061 *Paris* toises. And from these measures we conclude that the circumference of the earth is 123249600, and its semi-diameter 19615800 *Paris* feet, upon the supposition that the earth is of a spherical figure.

In the latitude of *Paris* a heavy body falling in a second of time describes 15 *Paris* feet, 1 inch, 1⅞ line, as above, that is, 2173 lines ⅞. The weight of the body is diminished by the weight of the ambient air. Let us suppose the weight lost thereby to be $\frac{1}{11000}$ part of the whole weight; then that heavy body falling *in vacuo* will describe a height of 2174 lines in one second of time.

A body in every sidereal day of 23^h. 56′ 4″ uniformly revolving in a circle at the distance of 19615800 feet from the centre, in one second of time describes an arc of 1433,46 feet; the versed sine of which is 0,05236561 feet, or 7,54064 lines. And therefore the force with which bodies descend in the latitude of *Paris* is to the centrifugal force of bodies in the equator arising from the diurnal motion of the earth as 2174 to 7,54064.

The centrifugal force of bodies in the equator is to the centrifugal force with which bodies recede directly from the earth in the latitude of *Paris* 48° 50′ 10″ in the duplicate proportion of the radius to the cosine of the latitude, that is, as 7,54064 to 3,267. Add this force to the force with which bodies descend by their weight in the latitude of *Paris,* and a body, in the latitude of *Paris,* falling by its whole undiminished force of gravity, in the time of one second, will describe 2177,267 lines, or 15 *Paris* feet, 1 inch, and 5,267 lines. And the total force of gravity in that latitude will be to the centrifugal force of bodies in the equator of the earth as 2177,267 to 7,54064, or as 289 to 1.

Wherefore if APBQ represent the figure of the earth, now no longer spherical, but generated by the rotation of an ellipsis about its lesser axis PQ; and ACQ *qca* a canal full of water, reaching from the pole Q*q* to the centre C*c*, and thence rising to the equator A*a*; the weight of the water in the leg of the canal AC*ca* will be to the weight of water in the other leg QC*cq* as 289 to 288, because the centrifugal force arising from the circular motion sustains and takes off one of the 289 parts of the weight (in the one leg), and the weight of 288 in the other sustains the rest. But by computation I find that if the matter of the earth was all uniform, and without any motion, and its axis PQ were to the diameter AB as 100 to 101, the force of gravity in the place Q towards the earth would be to the force of gravity in the same place Q towards a sphere described about the



centre C with the radius PC, or QC, as 126 to 125. And, by the same argument, the force of gravity in the place A towards the spheroid generated by the rotation of the ellipsis APBQ about the axis AB is to the force of gravity in the same place A, towards the sphere described about the centre C with the radius AC, as 125 to 126. But the force of gravity in the place A towards the earth is a mean proportional betwixt the forces of gravity towards the spheroid and this sphere; because the sphere, by having its diameter PQ diminished in the proportion of 101 to 100, is transformed into the figure of the earth; and this figure, by having a third diameter perpendicular to the two diameters AB and PQ diminished in the same proportion, is converted into the said spheroid; and the force of gravity in A, in either case, is diminished nearly in the same proportion. Therefore the force of gravity in A towards the sphere described about the centre C with the radius AC is to the force of gravity in A towards the earth as 126 to 125½. And the force of gravity in the place Q towards the sphere described about the centre C with the radius QC, is to the force of gravity in the place A towards the sphere described about the centre C, with the radius AC, in the proportion of the diameters (by Prop. LXXII, Book One), that is, as 100 to 101. If, therefore, we compound those three proportions 126 to 125, 126 to 125½, and 100 to 101, into one, the force of gravity in the place Q towards the earth will be to the force of gravity in the place A towards the earth as 126×126×100 to 125×125½×101; or as 501 to 500.

Now since the force of gravity in either leg of the canal AC*ca*, or QC*cq*, is as the distance of the places from the centre of the earth, if those

legs are conceived to be divided by transverse, parallel, and equidistant surfaces, into parts proportional to the wholes, the weights of any number of parts in the one leg AC*ca* will be to the weights of the same number of parts in the other leg as their magnitudes and the accelerative forces of their gravity conjunctly, that is, as 101 to 100, and 500 to 501, or as 505 to 501. And therefore if the centrifugal force of every part in the leg AC*ca*, arising from the diurnal motion, was to the weight of the same part as 4 to 505, so that from the weight of every part, conceived to be divided into 505 parts, the centrifugal force might take off four of those parts, the weights would remain equal in each leg, and therefore the fluid would rest in an equilibrium. But the centrifugal force of every part is to the weight of the same part as 1 to 289; that is, the centrifugal force, which should be $\frac{4}{505}$ parts of the weight, is only $\frac{1}{289}$ part thereof. And, therefore, I say, by the rule of proportion, that if the centrifugal force $\frac{4}{505}$ make the height of the water in the leg AC*ca* to exceed the height of the water in the leg QC*cq* by one $\frac{1}{100}$ part of its whole height, the centrifugal force $\frac{1}{289}$ will make the excess of the height in the leg AC*ca* only $\frac{1}{289}$ part of the height of the water in the other leg QC*cq;* and therefore the diameter of the earth at the equator, is to its diameter from pole to pole as 230 to 229. And since the mean semi-diameter of the earth, according to *Picart's* mensuration, is 19615800 *Paris* feet, or 3923,16 miles (reckoning 5000 feet to a mile), the earth will be higher at the equator than at the poles by 85472 feet, or $17\frac{1}{10}$ miles. And its height at the equator will be about 19658600 feet, and at the poles 19573000 feet.

If, the density and periodic time of the diurnal revolution remaining the same, the planet was greater or less than the earth, the proportion of the centrifugal force to that of gravity, and therefore also of the diameter betwixt the poles to the diameter at the equator, would likewise remain the same. But if the diurnal motion was accelerated or retarded in any proportion, the centrifugal force would be augmented or diminished nearly in the same duplicate proportion; and therefore the difference of the diameters will be increased or diminished in the same duplicate ratio very nearly. And if the density of the planet was augmented or diminished in any proportion, the force of gravity tending towards it would also be augmented or diminished in the same proportion: and the difference of the diameters contrariwise would be diminished in proportion as the force of gravity is augmented, and augmented in proportion as the force of gravity is diminished. Wherefore, since the earth, in respect of the fixed stars, revolves in $23^h.\ 56'$, but Jupiter in $9^h.\ 56'$, and the squares of their periodic times are as 29 to 5, and their densities as 400 to $94\frac{1}{2}$, the difference of the diameters of Jupiter will be to its lesser diameter as $\frac{29}{5} \times \frac{400}{94\frac{1}{2}} \times \frac{1}{229}$ to 1, or as 1 to $9\frac{1}{3}$, nearly. Therefore the diameter of Jupiter from east to west is to its diameter from pole to pole nearly as $10\frac{1}{3}$ to $9\frac{1}{3}$. Therefore since its greatest diameter is $37''$, its lesser diameter lying between the poles will be $33''\ 25'''$. Add thereto about $3''$ for the

irregular refraction of light, and the apparent diameters of this planet will become 40″ and 36″ 25‴; which are to each other as 11⅙ to 10⅙, very nearly. These things are so upon the supposition that the body of Jupiter is uniformly dense. But now if its body be denser towards the plane of the equator than towards the poles, its diameters may be to each other as 12 to 11, or 13 to 12, or perhaps as 14 to 13.

And *Cassini* observed in the year 1691 that the diameter of Jupiter reaching from east to west is greater by about a fifteenth part than the other diameter. Mr. *Pound* with his 123-feet telescope, and an excellent micrometer, measured the diameters of Jupiter in the year 1719 and found them as follow.

The Times.			Greatest diam.	Lesser diam.	The diam. to each other
	Day.	Hours.	Parts	Parts	
January	28	6	13,40	12,28	As 12 to 11
March	6	7	13,12	12,20	13¾ to 12¾
March	9	7	13,12	12,08	12⅔ to 11⅔
April	9	9	12,32	11,48	14½ to 13½

So that the theory agrees with the phænomena; for the planets are more heated by the sun's rays towards their equators, and therefore are a little more condensed by that heat than towards their poles.

Moreover, that there is a diminution of gravity occasioned by the diurnal rotation of the earth, and therefore the earth rises higher there than it does at the poles (supposing that its matter is uniformly dense), will appear by the experiments of pendulums related under the following Proposition.

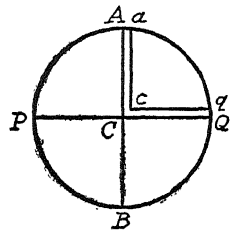
PROPOSITION XX. PROBLEM IV.

To find and compare together the weights of bodies in the different regions of our earth.

Because the weights of the unequal legs of the canal of water ACQ*qca* are equal; and the weights of the parts proportional to the whole legs, and alike situated in them, are one to another as the weights of the wholes, and therefore equal betwixt themselves; the weights of equal parts, and alike situated in the legs, will be reciprocally as the legs, that is, reciprocally as 230 to 229. And the case is the same in all homogeneous equal bodies alike situated in the legs of the canal. Their weights are reciprocally as the legs, that is, reciprocally as the distances of the bodies from the centre of the earth. Therefore if the bodies are situated in the uppermost parts of the canals, or on the surface of the earth, their weights will be one to another reciprocally as their distances from the centre. And, by the same argument, the weights in all other places round the whole surface of the earth are reciprocally as the distances of the places from

the centre; and, therefore, in the hypothesis of the earth's being a spheroid are given in proportion.

Whence arises this Theorem, that the increase of weight in passing from the equator to the poles is nearly as the versed sine of double the latitude; or, which comes to the same thing, as the square of the right sine of the latitude; and the arcs of the degrees of latitude in the meridian



increase nearly in the same proportion. And, therefore, since the latitude of *Paris* is 48° 50′, that of places under the equator 00° 00′, and that of places under the poles 90°; and the versed sines of double those arcs are 11334,00000 and 20000, the radius being 10000; and the force of gravity at the pole is to the force of gravity at the equator as 230 to 229; and the excess of the force of gravity at the pole to the force of gravity at the equator as 1 to 229; the excess of the force of gravity in the latitude of *Paris* will be to the force of gravity at the equator as $1 \times {}^{11334}\!/_{20000}$ to 229, or as 5667 to 2290000. And therefore the whole forces of gravity in those places will be one to the other as 2295667 to 2290000. Wherefore since the lengths of pendulums vibrating in equal times are as the forces of gravity, and in the latitude of *Paris,* the length of a pendulum vibrating seconds is 3 *Paris* feet, and 8½ lines, or rather because of the weight of the air, 8⅝ lines, the length of a pendulum vibrating in the same time under the equator will be shorter by 1,087 lines. And by a like calculus the table on the following page is made.

By this table, therefore, it appears that the inequality of degrees is so small that the figure of the earth, in geographical matters, may be considered as spherical; especially if the earth be a little denser towards the plane of the equator than towards the poles.

Now several astronomers, sent into remote countries to make astronomical observations, have found that pendulum clocks do accordingly move slower near the equator than in our climates. And, first of all, in the year 1672 M. *Richer* took notice of it in the island of *Cayenne;* for when, in the month of *August,* he was observing the transits of the fixed stars over the meridian, he found his clock to go slower than it ought in respect of the mean motion of the sun at the rate of 2′ 28″ a day. Therefore, fitting up a simple pendulum to vibrate in seconds, which were measured by an excellent clock, he observed the length of that simple pendulum; and this he did over and over every week for ten months together. And upon his return to *France,* comparing the length of that pendulum with the length

Latitude of the place	Length of the pendulum	Measure of one degree in the meridian
Deg.	Feet Lines.	Toises.
0	3 · 7,468	56637
5	3 · 7,482	56642
10	3 · 7,526	56659
15	3 · 7,596	56687
20	3 · 7,692	56724
25	3 · 7,812	56769
30	3 · 7,948	56823
35	3 · 8,099	56882
40	3 · 8,261	56945
1	3 · 8,294	56958
2	3 · 8,327	56971
3	3 · 8,361	56984
4	3 · 8,394	56997
45	3 · 8,428	57010
6	3 · 8,461	57022
7	3 · 8,494	57035
8	3 · 8,528	57048
9	3 · 8,561	57061
50	3 · 8,594	57074
55	3 · 8,756	57137
60	3 · 8,907	57196
65	3 · 9,044	57250
70	3 · 9,162	57295
75	3 · 9,258	57332
80	3 · 9,329	57360
85	3 · 9,372	57377
90	3 · 9,387	57382

of the pendulum at *Paris* (which was 3 *Paris* feet and 8⅗ lines), he found it shorter by 1¼ line.

Afterwards, our friend Dr. *Halley,* about the year 1677, arriving at the island of St. *Helena,* found his pendulum clock to go slower there than at *London* without marking the difference. But he shortened the rod of his clock by more than the ⅛ of an inch, or 1½ line; and to effect this, because the length of the screw at the lower end of the rod was not sufficient, he interposed a wooden ring betwixt the nut and the ball.

Then, in the year 1682, M. *Varin* and M. *des Hayes* found the length of a simple pendulum vibrating in seconds at the Royal Observatory of *Paris* to be 3 feet and 8⅝ lines. And by the same method in the island of *Goree,* they found the length of an isochronal pendulum to be 3 feet and 6⅝ lines, differing from the former by two lines. And in the same year, going to the islands of *Guadaloupe* and *Martinico,* they found that the length of an isochronal pendulum in those islands was 3 feet and 6½ lines.

After this, M. *Couplet,* the son, in the month of *July* 1697, at the Royal Observatory of *Paris,* so fitted his pendulum clock to the mean motion of

the sun that for a considerable time together the clock agreed with the motion of the sun. In *November* following, upon his arrival at *Lisbon,* he found his clock to go slower than before at the rate of 2′ 13″ in 24 hours. And next *March* coming to *Paraiba,* he found his clock to go slower than at *Paris,* and at the rate 4′ 12″ in 24 hours; and he affirms, that the pendulum vibrating in seconds was shorter at *Lisbon* by 2½ lines, and at *Paraiba* by 3⅔ lines, than at *Paris.* He had done better to have reckoned those differences 1⅓ and 2⅔: for these differences correspond to the differences of the times 2′ 13″ and 4′ 12″. But this gentleman's observations are so gross, that we cannot confide in them.

In the following years, 1699 and 1700, M. *des Hayes,* making another voyage to *America,* determined that in the island of *Cayenne* and *Granada* the length of the pendulum vibrating in seconds was a small matter less than 3 feet and 6½ lines; that in the island of St. *Christophers* it was 3 feet and 6¾ lines; and in the island of St. *Domingo* 3 feet and 7 lines.

And in the year 1704, P. *Feuillé,* at *Puerto Bello* in *America,* found that the length of the pendulum vibrating in seconds was 3 *Paris* feet, and only 5⁷⁄₁₂ lines, that is, almost 3 lines shorter than at *Paris;* but the observation was faulty. For afterward, going to the island of *Martinico,* he found the length of the isochronal pendulum there 3 *Paris* feet and 5¹⁰⁄₁₂ lines.

Now the latitude of *Paraiba* is 6° 38′ south; that of *Puerto Bello* 9° 33′ north; and the latitudes of the islands *Cayenne, Goree, Guadaloupe, Martinico, Granada,* St. *Christophers,* and St. *Domingo* are respectively 4° 55′, 14° 40″, 15° 00′, 14° 44′, 12° 06′, 17° 19′, and 19° 48′ north. And the excesses of the length of the pendulum at *Paris* above the lengths of the isochronal pendulums observed in those latitudes are a little greater than by the table of the lengths of the pendulum before computed. And therefore the earth is a little higher under the equator than by the preceding calculus, and a little denser at the centre than in mines near the surface, unless, perhaps, the heats of the torrid zone have a little extended the length of the pendulums.

For M. *Picart* has observed that a rod of iron, which in frosty weather in the winter season was one foot long, when heated by fire was lengthened into one foot and ¼ line. Afterward M. *de la Hire* found that a rod of iron, which in the like winter season was 6 feet long, when exposed to the heat of the summer sun, was extended into 6 feet and ⅔ line. In the former case the heat was greater than in the latter; but in the latter it was greater than the heat of the external parts of a human body; for metals exposed to the summer sun acquire a very considerable degree of heat. But the rod of a pendulum clock is never exposed to the heat of the summer sun, nor ever acquires a heat equal to that of the external parts of a human body; and, therefore, though the 3 feet rod of a pendulum clock will indeed be a little longer in the summer than in the winter season, yet the difference will scarcely amount to ¼ line. Therefore the total difference of the lengths of isochronal pendulums in different climates cannot be ascribed to the difference of heat; nor indeed to the

mistakes of the *French* astronomers. For although there is not a perfect agreement betwixt their observations, yet the errors are so small that they may be neglected; and in this they all agree, that isochronal pendulums are shorter under the equator than at the Royal Observatory of *Paris,* by a difference not less than 1¼ line nor greater than 2⅔ lines. By the observations of M. *Richer,* in the island of *Cayenne,* the difference was 1¼ line. That difference, being corrected by those of M. *des Hayes,* becomes 1½ line or 1¾ line. By the less accurate observations of others, the same was made about two lines. And this disagreement might arise partly from the errors of the observations, partly from the dissimilitude of the internal parts of the earth, and the height of mountains; partly from the different heats of the air.

I take an iron rod of 3 feet long to be shorter by a sixth part of one line in winter time with us here in *England* than in the summer. Because of the great heats under the equator, subduct this quantity from the difference of one line and a quarter observed by M. *Richer,* and there will remain one line $\frac{1}{12}$, which agrees very well with $1\frac{87}{1000}$ line collected, by the theory a little before. M. *Richer* repeated his observations, made in the island of *Cayenne,* every week for ten months together, and compared the lengths of the pendulum which he had there noted in the iron rods with the lengths thereof which he observed in *France.* This diligence and care seems to have been wanting to the other observers. If this gentleman's observations are to be depended on, the earth is higher under the equator than at the poles, and that by an excess of about 17 miles; as appeared above by the theory.

PROPOSITION XXIV. THEOREM XIX.

That the flux and reflux of the sea arise from the actions of the sun and moon.

By Cor. 19 and 20, Prop. LXVI, Book One, it appears that the waters of the sea ought twice to rise and twice to fall every day, as well lunar as solar; and that the greatest height of the waters in the open and deep seas ought to follow the appulse of the luminaries to the meridian of the place by a less interval than 6 hours; as happens in all that eastern tract of the *Atlantic* and *Æthiopic* seas between *France* and the *Cape of Good Hope;* and on the coasts of *Chili* and *Peru* in the *South Sea;* in all which shores the flood falls out about the second, third, or fourth hour, unless where the motion propagated from the deep ocean is by the shallowness of the channels, through which it passes to some particular places, retarded to the fifth, sixth, or seventh hour, and even later. The hours I reckon from the appulse of each luminary to the meridian of the place, as well under as above the horizon; and by the hours of the lunar day I understand the 24th parts of that time which the moon, by its apparent diurnal motion, employs to come about again to the meridian of the place which it left the day before. The force of the sun or moon in raising the sea is

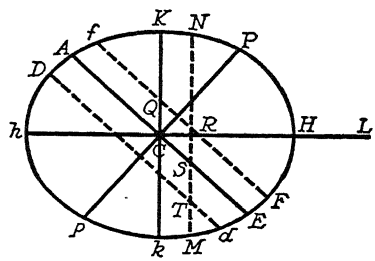
greatest in the appulse of the luminary to the meridian of the place; but the force impressed upon the sea at that time continues a little while after the impression, and is afterwards increased by a new though less force still acting upon it. This makes the sea rise higher and higher, till this new force becoming too weak to raise it any more, the sea rises to its greatest height. And this will come to pass, perhaps, in one or two hours, but more frequently near the shores in about three hours, or even more, where the sea is shallow.

The two luminaries excite two motions, which will not appear distinctly, but between them will arise one mixed motion compounded out of both. In the conjunction or opposition of the luminaries their forces will be conjoined, and bring on the greatest flood and ebb. In the quadratures the sun will raise the waters which the moon depresses, and depress the waters which the moon raises, and from the difference of their forces the smallest of all tides will follow. And because (as experience tells us) the force of the moon is greater than that of the sun, the greatest height of the waters will happen about the third lunar hour. Out of the syzygies and quadratures, the greatest tide, which by the single force of the moon ought to fall out at the third lunar hour, and by the single force of the sun at the third solar hour, by the compounded forces of both must fall out in an intermediate time that approaches nearer to the third hour of the moon than to that of the sun. And, therefore, while the moon is passing from the syzygies to the quadratures, during which time the 3d hour of the sun precedes the 3d hour of the moon, the greatest height of the waters will also precede the 3d hour of the moon, and that, by the greatest interval, a little after the octants of the moon; and, by like intervals, the greatest tide will follow the 3d lunar hour, while the moon is passing from the quadratures to the syzygies. Thus it happens in the open sea; for in the mouths of rivers the greater tides come later to their height.

But the effects of the luminaries depend upon their distances from the earth; for when they are less distant, their effects are greater, and when more distant, their effects are less, and that in the triplicate proportion of their apparent diameter. Therefore it is that the sun, in the winter time, being then in its perigee, has a greater effect, and makes the tides in the syzygies something greater, and those in the quadratures something less than in the summer season; and every month the moon, while in the perigee, raises greater tides than at the distance of 15 days before or after, when it is in its apogee. Whence it comes to pass that two highest tides do not follow one the other in two immediately succeeding syzygies.

The effect of either luminary doth likewise depend upon its declination or distance from the equator; for if the luminary was placed at the pole, it would constantly attract all the parts of the waters without any intension or remission of its action, and could cause no reciprocation of motion. And, therefore, as the luminaries decline from the equator towards either pole, they will, by degrees, lose their force, and on this account will

excite lesser tides in the solstitial than in the equinoctial syzygies. But in the solstitial quadratures they will raise greater tides than in the quadratures about the equinoxes; because the force of the moon, then situated in the equator, most exceeds the force of the sun. Therefore the greatest tides fall out in those syzygies, and the least in those quadratures, which happen about the time of both equinoxes: and the greatest tide in the syzygies is always succeeded by the least tide in the quadratures, as we find by experience. But, because the sun is less distant from the earth in winter than in summer, it comes to pass that the greatest and least tides more frequently appear before than after the vernal equinox, and more frequently after than before the autumnal.



Moreover, the effects of the luminaries depend upon the latitudes of places. Let A*p*EP represent the earth covered with deep waters; C its centre; P, *p* its poles; AE the equator; F any place without the equator; F*f* the parallel of the place; D*d* the correspondent parallel on the other side of the equator; L the place of the moon three hours before; H the place of the earth directly under it; *h* the opposite place; K, *k* the places at 90 degrees distance; CH, C*h*, the greatest heights of the sea from the centre of the earth; and CK, C*k*, its least heights: and if with the axes H*h*, K*k*, an ellipsis is described, and by the revolution of that ellipsis about its longer axis H*h* a spheroid HPK*hpk* is formed, this spheroid will nearly represent the figure of the sea; and CF, C*f*, CD, C*d*, will represent the heights of the sea in the places F*f*, D*d*. But farther; in the said revolution of the ellipsis any point N describes the circle NM cutting the parallels F*f*, D*d*, in any places RT, and the equator AE in S; CN will represent the height of the sea in all those places R, S, T, situated in this circle. Wherefore, in the diurnal revolution of any place F, the greatest flood will be in F, at the third hour after the appulse of the moon to the meridian above the horizon; and afterwards the greatest ebb in Q, at the third hour after the setting of the moon; and then the greatest flood in *f*, at the third hour after the appulse of the moon to the meridian under the horizon; and, lastly, the greatest ebb in Q, at the third hour after the rising of the moon; and the latter flood in *f* will be less than the preceding flood in F. For the whole sea is divided into two hemispherical floods, one in the hemisphere KH*k* on the north side, the other in the opposite hemisphere K*hk*, which we may therefore call the northern and the southern floods.

These floods, being always opposite the one to the other, come by turns to the meridians of all places, after an interval of 12 lunar hours. And seeing the northern countries partake more of the northern flood, and the southern countries more of the southern flood, thence arise tides, alternately greater and less in all places without the equator, in which the luminaries rise and set. But the greatest tide will happen when the moon declines towards the vertex of the place, about the third hour after the appulse of the moon to the meridian above the horizon; and when the moon changes its declination *to the other side of the equator,* that which was the greater tide will be changed into a lesser. And the greatest difference of the floods will fall out about the times of the solstices; especially if the ascending node of the moon is about the first of Aries. So it is found by experience that the morning tides in winter exceed those of the evening, and the evening tides in summer exceed those of the morning; at *Plymouth* by the height of one foot, but at *Bristol* by the height of 15 inches, according to the observations of *Colepress* and *Sturmy.*

But the motions which we have been describing suffer some alteration from that force of reciprocation, which the waters, being once moved, retain a little while *by their vis insita.* Whence it comes to pass that the tides may continue for some time, though the actions of the luminaries should cease. This power of retaining the impressed motion lessens the difference of the alternate tides, and makes those tides which immediately succeed after the syzygies greater, and those which follow next after the quadratures less. And hence it is that the alternate tides at *Plymouth* and *Bristol* do not differ much more one from the other than by the height of a foot or 15 inches, and that the greatest tides of all at those ports are not the first but the third after the syzygies. And, besides, all the motions are retarded in their passage through shallow channels, so that the greatest tides of all, in some straits and mouths of rivers, are the fourth or even the fifth after the syzygies.

Farther, it may happen that the tide may be propagated from the ocean through different channels towards the same port, and may pass quicker through some channels than through others; in which case the same tide, divided into two or more succeeding one another, may compound new motions of different kinds. Let us suppose two equal tides flowing towards the same port from different places, the one preceding the other by 6 hours; and suppose the first tide to happen at the third hour of the appulse of the moon to the meridian of the port. If the moon at the time of the appulse to the meridian was in the equator, every 6 hours alternately there would arise equal floods, which, meeting with as many equal ebbs, would so balance one the other that for that day the water would stagnate and remain quiet. If the moon then declined from the equator, the tides in the ocean would be alternately greater and less, as was said; and from thence two greater and two lesser tides would be alternately propagated towards that port. But the two greater floods would make the greatest height of the waters to fall out in the middle time

betwixt both; and the greater and lesser floods would make the waters to rise to a mean height in the middle time between them, and in the middle time between the two lesser floods the waters would rise to their least height. Thus in the space of 24 hours the waters would come, not twice, as commonly, but once only to their greatest, and once only to their least height; and their greatest height, if the moon declined towards the elevated pole, would happen at the 6th or 30th hour after the appulse of the moon to the meridian; and when the moon changed its declination, this flood would be changed into an ebb. An example of all which Dr. *Halley* has given us, from the observations of seamen in the port of *Batsham,* in the kingdom of *Tunquin,* in the latitude of 20° 50′ north. In that port, on the day which follows after the passage of the moon over the equator, the waters stagnate: when the moon declines to the north, they begin to flow and ebb, not twice, as in other ports, but once only every day: and the flood happens at the setting, and the greatest ebb at the rising of the moon. This tide increases with the declination of the moon till the 7th or 8th day; then for the 7 or 8 days following it decreases at the same rate as it had increased before, and ceases when the moon changes its declination, crossing over the equator to the south. After which the flood is immediately changed into an ebb; and thenceforth the ebb happens at the setting and the flood at the rising of the moon; till the moon, again passing the equator, changes its declination. There are two inlets to this port and the neighboring channels, one from the seas of *China,* between the continent and the island of *Leuconia;* the other from the *Indian* sea, between the continent and the island of *Borneo.* But whether there be really two tides propagated through the said channels, one from the *Indian* sea in the space of 12 hours, and one from the sea of *China* in the space of 6 hours, which therefore happening at the 3d and 9th lunar hours, by being compounded together, produce those motions; or whether there be any other circumstances in the state of those seas, I leave to be determined by observations on the neighbouring shores.

Thus I have explained the causes of the motions of the moon and of the sea.

GENERAL SCHOLIUM

Bodies projected in our air suffer no resistance but from the air. Withdraw the air, as is done in Mr. *Boyle's* vacuum, and the resistance ceases; for in this void a bit of fine down and a piece of solid gold descend with equal velocity. And the parity of reason must take place in the celestial spaces above the earth's atmosphere; in which spaces, where there is no air to resist their motions, all bodies will move with the greatest freedom; and the planets and comets will constantly pursue their revolutions in orbits given in kind and position, according to the laws above explained; but though these bodies may, indeed, persevere in their orbits by the mere laws of gravity, yet they could by no means have at first derived the regular position of the orbits themselves from those laws.

The six primary planets are revolved about the sun in circles concentric with the sun, and with motions directed towards the same parts, and almost in the same plane. Ten moons are revolved about the earth, Jupiter and Saturn, in circles concentric with them, with the same direction of motion, and nearly in the planes of the orbits of those planets; but it is not to be conceived that mere mechanical causes could give birth to so many regular motions, since the comets range over all parts of the heavens in very eccentric orbits; for by that kind of motion they pass easily through the orbs of the planets, and with great rapidity; and in their aphelions, where they move the slowest, and are detained the longest, they recede to the greatest distances from each other, and thence suffer the least disturbance from their mutual attractions. This most beautiful system of the sun, planets, and comets could only proceed from the counsel and dominion of an intelligent and powerful Being. And if the fixed stars are the centres of other like systems, these, being formed by the like wise counsel, must be all subject to the dominion of One; especially since the light of the fixed stars is of the same nature with the light of the sun, and from every system light passes into all the other systems: and lest the systems of the fixed stars should, by their gravity, fall on each other mutually, he hath placed those systems at immense distances one from another.

Hitherto we have explained the phænomena of the heavens and of our sea by the power of gravity, but have not yet assigned the cause of this power. This is certain, that it must proceed from a cause that penetrates to the very centres of the sun and planets, without suffering the least diminution of its force; that operates not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes use to do), but according to the quantity of the solid matter which they contain, and propagates its virtue on all sides to immense distances, decreasing always in the duplicate proportion of the distances. Gravitation towards the sun is made up out of the gravitations towards the several particles of which the body of the sun is composed; and in receding from the sun decreases accurately in the duplicate proportion of the distances as far as the orb of Saturn, as evidently appears from the quiescence of the aphelions of the planets; nay, and even to the remotest aphelions of the comets, if those aphelions are also quiescent. But hitherto I have not been able to discover the cause of those properties of gravity from phænomena, and I frame no hypotheses; for whatever is not deduced from the phænomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phænomena, and afterwards rendered general by induction. Thus it was that the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and of gravitation, were discovered. And to us it is enough that gravity does really exist, and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies, and of our sea.

And now we might add something concerning a certain most subtle

Spirit which pervades and lies hid in all gross bodies; by the force and action of which Spirit the particles of bodies mutually attract one another at near distances, and cohere, if contiguous; and electric bodies operate to greater distances, as well repelling as attracting the neighbouring corpuscles; and light is emitted, reflected, refracted, inflected, and heats bodies; and all sensation is excited, and the members of animal bodies move at the command of the will, namely, by the vibrations of this Spirit, mutually propagated along the solid filaments of the nerves, from the outward organs of sense to the brain, and from the brain into the muscles. But these are things that cannot be explained in few words, nor are we furnished with that sufficiency of experiments which is required to an accurate determination and demonstration of the laws by which this electric and elastic Spirit operates.

END OF THE MATHEMATICAL PRINCIPLES

THE ATOMIC THEORY

by

JOHN DALTON

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JOHN DALTON

1766–1844

AT THE HEIGHT OF HIS FAME, John Dalton wrote the following note in the autograph album belonging to a friend of his:

The writer of this was born at the village of Eaglesfield, about two miles west of Cockermouth, Cumberland. Attended the village schools, there and in the neighborhood, till eleven years of age, at which period he had gone through a course of mensuration, surveying, navigation, etc.; began about twelve to teach the village school and continued it about two years; afterwards was occasionally employed in husbandry for a year or more; removed to Kendal at fifteen years of age as assistant in a boarding school; remained in that capacity for three or four years; then undertook the same school as principal and continued it for eight years; whilst at Kendal employed his leisure in studying Latin, Greek, French and the mathematics, with natural philosophy; removed thence to Manchester in 1793 as tutor in mathematics and natural philosophy in the New College; was six years in that engagement and after was employed as private and public teacher of mathematics and chemistry in Manchester, but occasionally by invitation in London, Edinburgh, Glasgow, Birmingham and Leeds.

Oct. 22, 1832 JOHN DALTON

These bare bones of biography can fortunately be clothed with flesh. Dalton was born in 1766, one of the six children of Joseph and Deborah Dalton, humble Quakers. Joseph Dalton was a hand-loom weaver and the farmer of a small patch of land which he owned. Nothing in the family life conduced to special refinement save the simple Quaker faith. The elder Daltons had benefited by neither formal education nor wealth; they differed little from their neighbors, most of them also plain, honest, rugged small farmers and tradespeople. The town schools provided only such provender as John Dalton was able to exhaust in a half dozen years, and required of a

teacher no further qualifications than Dalton was able to offer when he was twelve. One may question his success as a teacher at that age; it was evidently sufficient to persuade him to elect teaching as his profession.

When he journeyed to Kendal to assist in a boarding school, Dalton went on the invitation of a cousin who was head of the school. He had, as he reports, leisure there to study languages, mathematics, and natural philosophy. He had also leisure to contribute vapid answers to vapid questions in two periodicals, the *Ladies' Diary* and the *Gentleman's Diary*. For example, to the question, Can one who has loved sincerely love a second time? he replied with a curiously silly essay.

Much more important, during these Kendal years Dalton met John Gough. Gough was twice Dalton's age, and as a result of smallpox had been blind from his infancy. Yet he was a good classical scholar, and it was he who provoked Dalton to study Greek and Latin. Well-informed about the science of the day, he thought scientifically; and he taught his younger friend to think similarly. He persuaded him to make and record his first scientific observations, a series of local weather data collected with the aid of homemade barometers, thermometers, and hygroscopes. During these same years, while Dalton was intermittently considering law and medicine as possible professions, he also collected and dried botanical specimens, collected insects, experimented with his own body to determine what proportion of food and drink ingested passed off as "insensible perspiration."

In 1793, through Gough's influence, Dalton was appointed tutor in mathematics and natural philosophy at New College in Manchester. Almost at once he published his *Meteorological Observations and Essays* (Manchester, 1793). This opens with an account of an aurora borealis he had observed in 1787 and a discussion of the causes and effects of auroras. One essay considers the rise and fall of the barometer and the causes therefor. The most important essay, historically, is the one on evaporation, for in it he first states the idea now known as Dalton's law, the law of partial pressures.

In 1794 the Manchester Literary and Philosophical Society elected Dalton to membership. The first paper he presented to the Society he titled "On Vision of Colour," and in it he used data from his own and his brother's experiences. They were both color-blind, as they had discovered when they brought their mother, as a good Quaker present, a pair of silk stockings of brilliant crimson. Later he presented to the Society papers on rain and dew, on heat conduction, on "Heat and Cold Produced by Mechanical Condensation and

Rarefaction of Air." In all of these papers Dalton relied for data on his own loose experiments and his own inaccurate instruments. His numerical results have not been confirmed by later students. Yet the essays are valuable, for the experiments are most sagaciously interpreted, and Dalton exercised in them his wonderful faculty for happy generalization. Thus, in 1803, in a paper "On the Tendency of Elastic Fluids to Diffuse Through Each Other," from quite insufficient data he evolved the final form of the law of partial pressures. Similarly, in a paper on the expansion of gases by heat, he anticipated by six months Gay-Lussac's conclusions.

During these years in Manchester, Dalton was teaching mechanics, algebra, geometry, bookkeeping, chemistry, and natural philosophy to private students as well as in New College. He traveled very little—only occasionally to Bristol and to London, which he thought the "most disagreeable place for one of a contemplative turn to exist in"—and his contact with the intellectual and scientific world was wholly through the books available to him in the free library of Manchester. Yet his papers were attracting such attention that in 1803 he was invited to give a course of lectures at the Royal Institution in London, and his teaching was drawing to him so many private pupils that he withdrew from New College.

Between 1803 and 1820, after which Dalton's powers faded and his production diminished, he prepared studies on fog, on alloys, on sulphuric ether, on respiration, and on animal heat. Most important, he developed his atomic theory. He first presented his ideas on atoms in a series of lectures given in Glasgow in 1807; and in a second course of lectures at the Royal Institution in London, in 1809–10, he explained how he had come to his conclusions. The real publication came, however, in the first volume of his *New System of Chemical Philosophy,* 1808. From this volume pertinent passages are here reprinted.

In person, Dalton was of middle height, robust, muscular, and awkward. His mouth was firm, his voice gruff, his chin massive. He was said to resemble Newton. His mode of living was always quiet, adjusted to the contemplative life he preferred. For thirty years he occupied the same lodgings in Manchester (he never married) going thence daily to his rooms at the Literary and Philosophical Society to receive his pupils and do his own experimenting. On Sundays he faithfully attended the Quaker services, and on Thursdays he played a weekly game of bowls.

Dalton's theory of the atomic composition of all matter won quick recognition and acceptance. It earned him such

high regard from the scientific and academic world that in his last years honors showered upon him. In 1816 he was elected a corresponding member of the French Academy of Science; in 1822 he was elected Fellow of the Royal Society; in 1826 he was the first recipient of the annual royal medal and prize recently established by George IV; in 1832 Oxford made him a Doctor of Common Law; in 1833 the government awarded him a pension for life, and in the announcement of the grant he was named "one of the greatest legislators of chemical science." He held also a degree as Doctor of Law from Edinburgh, and memberships in learned societies in Munich, Moscow, and Berlin. When he visited Paris in 1822, Biot, Ampère, Arago, Fresnel, Laplace, Cuvier, and other French scientists combined their efforts to honor him.

Many of Dalton's ideas in chemistry have been superseded. His theories of heat are as out-of-date as his use of *elastic fluid* for "gas," *azotic gas* for "nitrogen," *oxygenous gas* for "oxygen," et cetera. His fame is nevertheless secure. It rests upon his discovery of a simple principle, universally applicable to the facts of chemistry—that elements combine always in fixed proportions. Sir Humphry Davy rightly said that in laying the foundation for future labors, Dalton's labors in chemistry resembled those of Kepler in astronomy.

THE ATOMIC THEORY

I. ON THE CONSTITUTION OF BODIES

THERE ARE three distinctions in the kinds of bodies, or three states, which have more especially claimed the attention of philosophical chemists; namely, those which are marked by the terms *elastic fluids, liquids, and solids*. A very familiar instance is exhibited to us in water, of a body, which, in certain circumstances, is capable of assuming all the three states. In steam we recognise a perfectly elastic fluid, in water, a perfect liquid, and in ice, a complete solid. These observations have tacitly led to the conclusion which seems universally adopted, that all bodies of sensible magnitude, whether liquid or solid, are constituted of a vast number of extremely small particles, or atoms of matter bound together by a force of attraction, which is more or less powerful according to circumstances, and which, as it endeavours to prevent their separation, is very properly called, in that view, *attraction of cohesion;* but as it collects them from a dispersed state (as from steam into water) it is called, *attraction of aggregation,* or, more simply, *affinity*. Whatever names it may go by, they still signify one and the same power. It is not my design to call in question this conclusion, which appears completely satisfactory; but to shew that we have hitherto made no use of it, and that the consequence of the neglect has been a very obscure view of chemical agency, which is daily growing more so in proportion to the new lights attempted to be thrown upon it.

Whether the ultimate particles of a body, such as water, are all alike, that is, of the same figure, weight, &c., is a question of some importance. From what is known, we have no reason to apprehend a diversity in these particulars: if it does exist in water, it must equally exist in the elements constituting water, namely, hydrogen and oxygen. Now it is scarcely possible to conceive how the aggregates of dissimilar particles should be so uniformly the same. If some of the particles of water were heavier than others, if a parcel of the liquid on any occasion were constituted principally of these heavier particles, it must be supposed to affect the specific gravity of the mass, a circumstance not known. Similar observations may be made on other substances. Therefore we may conclude that *the ultimate particles of all homogeneous bodies are perfectly alike in weight, figure, &c.* In other words, every particle of water is like every other particle of water; every particle of hydrogen is like every other particle of hydrogen, &c.

Besides the force of attraction, which, in one character or another,

belongs universally to ponderable bodies, we find another force that is likewise universal, or acts upon all matter which comes under our cognisance, namely, a force of repulsion. This is now generally, and I think properly, ascribed to the agency of heat. An atmosphere of this subtile fluid constantly surrounds the atoms of all bodies, and prevents them from being drawn into actual contact. This appears to be satisfactorily proved by the observation that the bulk of a body may be diminished by abstracting some of its heat; but it should seem that enlargement and diminution of bulk depend perhaps more on the arrangement than on the size of the ultimate particles.

We are now to consider how these two great antagonist powers of attraction and repulsion are adjusted, so as to allow of the three different states of *elastic fluids, liquids, and solids.* We shall divide the subject into four Sections; namely, first, *on the constitution of pure elastic fluids;* second, *on the constitution of mixed elastic fluids;* third, *on the constitution of liquids,* and fourth, *on the constitution of solids.*

Section 1. On the Constitution of Pure Elastic Fluids

A pure elastic fluid is one, the constituent particles of which are all alike, or in no way distinguishable. Steam, or aqueous vapour, hydrogenous gas, oxygenous gas, azotic gas, and several others are of this kind. These fluids are constituted of particles possessing very diffuse atmospheres of heat, the capacity or bulk of the atmosphere being often one or two thousand times that of the particle in a liquid or solid form. Whatever therefore may be the shape or figure of the solid atom abstractedly, when surrounded by such an atmosphere it must be globular; but as all the globules in any small given volume are subject to the same pressure, they must be equal in bulk, and will therefore be arranged in horizontal strata, like a pile of shot. A volume of elastic fluid is found to expand whenever the pressure is taken off. This proves that the repulsion exceeds the attraction in such case. The absolute attraction and repulsion of the particles of an elastic fluid, we have no means of estimating, though we can have little doubt but that the cotemporary energy of both is great; but the excess of the repulsive energy above the attractive can be estimated, and the law of increase and diminution be ascertained in many cases. Thus, in steam, the density may be taken at $\frac{1}{1728}$ that of water; consequently each particle of steam has 12 times the diameter that one of water has, and must press upon 144 particles of a watery surface; but the pressure upon each is equivalent to that of a column of water of 34 feet; therefore the excess of the elastic force in a particle of steam is equal to the weight of a column of particles of water, whose height is $34 \times 144 = 4896$ feet. And further, this elastic force decreases as the distance of the particles increases. With respect to steam and other elastic fluids then, the force of cohesion is entirely counteracted by that of repulsion, and the only force which is efficacious to move the particles is the excess of the repulsion above the attraction. Thus, if the attraction be as 10 and

the repulsion as 12, the effective repulsive force is as 2. It appears, then that an elastic fluid, so far from requiring any force to separate its particles, always requires a force to retain them in their situation, or to prevent their separation.

Some elastic fluids, as hydrogen, oxygen, &c., resist any pressure that has yet been applied to them. In such then it is evident the repulsive force of heat is more than a match for the affinity of the particles and the external pressure united. To what extent this would continue we cannot say; but from analogy we might apprehend that a still greater pressure would succeed in giving the attractive force the superiority, when the elastic fluid would become a liquid or solid. In other elastic fluids, as steam, upon the application of compression to a certain degree, the elasticity apparently ceases altogether, and the particles collect in small drops of liquid, and fall down. This phenomenon requires explanation.

The constitution of a liquid, as water, must then be conceived to be that of an aggregate of particles, exercising in a most powerful manner the forces of attraction and repulsion, but nearly in an equal degree.—Of this more in the sequel.

Section 2. On the Constitution of Mixed Elastic Fluids

When two or more elastic fluids, whose particles do not unite chemically upon mixture, are brought together, one measure of each, they occupy the space of two measures, but become uniformly diffused through each other, and remain so, whatever may be their specific gravities. The fact admits of no doubt; but explanations have been given in various ways, and none of them completely satisfactory. As the subject is one of primary importance in forming a system of chemical principles, we must enter somewhat more fully into the discussion.

Dr. Priestley was one of the earliest to notice the fact: it naturally struck him with surprise that two elastic fluids, having apparently no affinity for each other, should not arrange themselves according to their specific gravities, as liquids do in like circumstances. Though he found this was not the case after the elastic fluids had once been thoroughly mixed, yet he suggests it as probable that if two of such fluids could be exposed to each other without agitation, the one specifically heavier would retain its lower situation. He does not so much as hint at such gases being retained in a mixed state by affinity. With regard to his suggestion of two gases being carefully exposed to each other without agitation, I made a series of experiments expressly to determine the question. From these it seems to be decided that gases always intermingle and gradually diffuse themselves amongst each other, if exposed ever so carefully; but it requires a considerable time to produce a complete intermixture, when the surface of communication is small. This time may vary from a minute to a day or more, according to the quantity of the gases and the freedom of communication.

When or by whom the notion of mixed gases being held together

by chemical affinity was first propagated, I do not know; but it seems probable that the notion of water being dissolved in air led to that of air being dissolved in air.—Philosophers found that water gradually disappeared or evaporated in air, and increased its elasticity; but steam at a low temperature was known to be unable to overcome the resistance of the air, therefore the agency of affinity was necessary to account for the effect. In the permanently elastic fluids indeed, this agency did not seem to be so much wanted, as they are all able to support themselves; but the diffusion through each other was a circumstance which did not admit of an easy solution any other way. In regard to the solution of water in air, it was natural to suppose, nay, one might almost have been satisfied without the aid of experiment, that the different gases would have had different affinities for water, and that the quantities of water, dissolved in like circumstances, would have varied according to the nature of the gas. Saussure found however that there was no difference in this respect in the solvent powers of carbonic acid, hydrogen gas, and common air.—It might be expected that at least the *density* of the gas would have some influence upon its solvent powers, that air of half density would take half the water, or the quantity of water would diminish in some proportion to the density; but even here again we are disappointed; whatever be the rarefaction, if water be present, the vapour produces the same elasticity, and the hygrometer finally settles at extreme moisture, as in air of common density in like circumstances. These facts are sufficient to create extreme difficulty in the conception how any principle of affinity or *cohesion* between air and water can be the agent. It is truly astonishing that the same quantity of vapour should cohere to *one* particle of air in a given space as to *one thousand* in the same space. But the wonder does not cease here; a Torricellian vacuum dissolves water; and in this instance we have vapour existing independently of air at all temperatures; what makes it still more remarkable is, the vapour in such vacuum is precisely the same in quantity and force as in the like volume of any kind of air of extreme moisture.

These and other considerations which occurred to me some years ago were sufficient to make me altogether abandon the hypothesis of air dissolving water, and to explain the phenomena some other way, or to acknowledge they were inexplicable. In the autumn of 1801, I hit upon an idea which seemed to be exactly calculated to explain the phenomena of vapour; it gave rise to a great variety of experiments.

The distinguishing feature of the new theory was that the particles of one gas are not elastic or repulsive in regard to the particles of another gas, but only to the particles of their own kind. Consequently when a vessel contains a mixture of two such elastic fluids, each acts independently upon the vessel, with its proper elasticity, just as if the other were absent, whilst no mutual action between the fluids themselves is observed. This position most effectually provided for the existence of vapour of any temperature in the atmosphere, because it could have nothing but its own weight to support; and it was perfectly obvious why neither more

nor less vapour could exist in air of extreme moisture than in a vacuum of the same temperature. So far then the great object of the theory was attained. The law of the condensation of vapour in the atmosphere by cold was evidently the same on this scheme as that of the condensation of pure steam, and experience was found to confirm the conclusion at all temperatures. The only thing now wanting to completely establish the independent existence of aqueous vapour in the atmosphere was the conformity of other liquids to water, in regard to the diffusion and condensation of their vapour. This was found to take place in several liquids, and particularly in sulphuric ether, one which was most likely to shew any anomaly to advantage if it existed, on account of the great change of expansibility in its vapour at ordinary temperatures. The existence of vapour in the atmosphere and its occasional condensation were thus accounted for; but another question remained, how does it rise from a surface of water subject to the pressure of the atmosphere?

From the novelty, both in the theory and the experiments, and their importance, provided they were correct, the new facts and experiments were highly valued, some of the latter were repeated, and found correct, and none of the results, as far as I know, have been controverted; but the theory was almost universally misunderstood, and consequently reprobated. This must have arisen partly at least from my being too concise, and not sufficiently clear in its exposition.

Dr. Thomson was the first, as far as I know, who publicly animadverted upon the theory; this gentleman, so well known for his excellent *System of Chemistry,* observed in the first edition of that work that the theory would not account for the equal distribution of gases; but that, granting the supposition of one gas neither attracting nor repelling another, the two must still arrange themselves according to their specific gravity. But the most general objection to it was quite of a different kind; it was admitted that the theory was adapted so as to obtain the most uniform and permanent diffusion of gases; but it was urged that as one gas was as a vacuum to another, a measure of any gas being put to a measure of another, the two measures ought to occupy the space of one measure only. Finding that my views on the subject were thus misapprehended, I wrote an illustration of the theory, which was published in the 3d Vol. of *Nicholson's Journal,* for November, 1802. In that paper I endeavoured to point out the conditions of mixed gases more at large, according to my hypothesis; and particularly touched upon the discriminating feature of it, that of two particles of any gas A, repelling each other by the known stated law, whilst one or more particles of another gas B were interposed in a direct line, without at all affecting the reciprocal action of the said two particles of A. Or, if any particle of B were casually to come in contact with one of A, and press against it, this pressure did not preclude the cotemporary action of all the surrounding particles of A upon the one in contact with B. In this respect the mutual action of particles of the same gas was represented as resembling magnetic action, which is not disturbed by the intervention of a body not magnetic.

Berthollet in his *Chemical Statics* (1804) has given a chapter on the constitution of the atmosphere, in which he has entered largely into a discussion of the new theory. This celebrated chemist, upon comparing the results of experiments made by De Luc, Saussure, Volta, Lavoisier, Watt, &c., together with those of Gay-Lussac, and his own, gives his full assent to the fact that vapours of every kind increase the elasticity of each species of gas alike, and just as much as the force of the said vapours in vacuo; and not only so, but that the specific gravity of vapour in air and vapour in vacuo is in all cases the same (Vol. 1. Sect. 4). Consequently he adopts the theorem for finding the quantity of vapour which a given volume of air can dissolve, which I have laid down; namely,

$$s = \frac{p}{p-f}$$

where p represents the pressure upon a given volume (1) of dry air, expressed in inches of mercury, f = the force of the vapour in vacuo at the temperature, in inches of mercury, and s = the space which the mixture of air and vapour occupies under the given pressure, p, after saturation. So far therefore we perfectly agree: but he objects to the theory by which I attempt to explain these phenomena, and substitutes another of his own.

The first objection I shall notice is one that clearly shews Berthollet either does not understand or does not rightly apply the theory he opposes; he says, "If one gas occupied the interstices of another, as though they were vacancies, there would not be any augmentation of volume when aqueous or ethereal vapour was combined with the air; nevertheless there is one proportional to the quantity of vapour added: humidity should increase the specific gravity of the air, whereas it renders it specifically lighter, as has been already noticed by Newton." This is the objection which has been so frequently urged. Let a tall cylindrical glass vessel containing dry air be inverted over mercury, and a portion of the air drawn out by a syphon, till an equilibrium of pressure is established within and without; let a small portion of water, ether, &c., be then thrown up into the vessel; the vapour rises and occupies the interstices of the air as a void; but what is the obvious consequence? Why, the surface of the mercury being now pressed both by the dry air and by the new raised vapour is more pressed within than without, and an enlargement of the volume of air is unavoidable, in order to restore the equilibrium. Again, in the open air: suppose there were no aqueous atmosphere around the earth, only an azotic one = 23 inches of mercury, and an oxygenous one = 6 inches. The air being thus perfectly dry, evaporation would commence with great speed. The vapour first formed, being constantly urged to ascend by that below, and as constantly resisted by the air, must, in the first instance, dilate the other two atmospheres (for the ascending steam adds its force to the upward elasticity of the two gases, and in part alleviates their pressure, the necessary consequence of which

is dilatation). At last, when all the vapour has ascended that the temperature will admit of, the aqueous atmosphere attains an equilibrium; it no longer presses upon the other two, but upon the earth; the others return to their original density and pressure throughout. In this case, it is true, there would not be any augmentation of volume when aqueous vapour was combined with the air; humidity would increase the weight of the congregated atmospheres, but diminish their specific gravity under a given pressure. One would have thought that this solution of the phenomenon upon my hypothesis was too obvious to escape the notice of anyone in any degree conversant with pneumatic chemistry.

Another objection is derived from the very considerable time requisite for a body of hydrogen to descend into one of carbonic acid; if one gas were as a vacuum for another, why is the equilibrium not instantly established? This objection is certainly plausible; we shall consider it more at large hereafter.

In speaking of the pressure of the atmosphere retaining water in a liquid state, which I deny, Berthollet adopts the idea of Lavoisier, "that without it the moleculæ would be infinitely dispersed, and that nothing would limit their separation, unless their own weight should collect them to form an atmosphere." This, I may remark, is not the language dictated by a correct notion on the subject. Suppose our atmosphere were annihilated, and the waters on the surface of the globe were instantly expanded into steam; surely the action of gravity would collect the moleculæ into an atmosphere of similar constitution to the one we now possess; but suppose the whole mass of water evaporated amounted in weight to 30 inches of mercury, how could it support its own weight at the common temperature? It would in a short time be condensed into water merely by its weight, leaving a small portion, such as the temperature could support, amounting perhaps to half an inch of mercury in weight, as a permanent atmosphere, which would effectually prevent any more vapour from rising, unless there were an increase of temperature. Does not everyone know that water and other liquids can exist in a Torricellian vacuum at low temperatures solely by the pressure of vapour arising from them? What need then of the pressure of the atmosphere in order to prevent an excess of vapourisation?

The experiments of Fontana on the distillation of water and ether in close vessels containing air are adduced to prove that vapours do not penetrate air without resistance. This is true no doubt; vapour cannot make its way in such circumstances through a long and circuitous route without time, and if the external atmosphere keep the vessel cool, the vapour may be condensed by its sides, and fall down in a liquid form as fast as it is generated, without ever penetrating in any sensible quantity to its remote extremity.

Dr. Thomson, in the 3d Edition of his *System of Chemistry,* has entered into a discussion on the subject of mixed gases; he seems to comprehend the excellence and defects of my notions on these subjects, with great acuteness. He does not conclude with Berthollet that, on my

hypothesis, "there would not be any augmentation of volume when aqueous and ethereal vapour was combined with the air," which has been so common an objection. There is however one objection which this gentleman urges that shews he does not completely understand the mechanism of my hypothesis. At page 448, Vol. 3, he observes that from the principles of hydrostatics, "each particle of a fluid sustains the whole pressure. Nor can I perceive any reason why this principle should not hold, even on the supposition that Dalton's hypothesis is well founded." Upon this I would observe that when once an equilibrium is established in any mixture of gases, each particle of gas is pressed as if by the surrounding particles *of its own kind only.* It is in the renunciation of that hydrostatical principle that the leading feature of the theory consists. The lowest particle of oxygen in the atmosphere sustains the weight of all the particles of oxygen above it, and the weight of no other. It was therefore a maxim with me that every particle of gas is equally pressed in every direction, but the pressure arises from the particles of its own kind only. Indeed when a measure of oxygen is put to a measure of azote, at the moment the two surfaces come in contact, the particles of each gas press against those of the other with their full force; but the two gases get gradually intermingled, and the force which each particle has to sustain proportionally diminishes, till at last it becomes the same as that of the original gas dilated to twice its volume. The ratio of the forces is as the cube root of the spaces inversely; that is, $\sqrt[3]{2} : 1$, or as 1.26 : 1 nearly. In such a mixture as has just been mentioned, then, the common hypothesis supposes the pressure of each particle of gas to be 1.26; whereas mine supposes it only to be 1; but the sum of the pressure of both gases on the containing vessel, or any other surface, is exactly the same on both hypotheses.

With regard to the objection that one gas makes a more durable resistance to the entrance of another than it ought to do on my hypothesis: This occurred to me in a very early period of my speculations; I devised the train of reasoning which appeared to obviate the objection; but it being necessarily of a mathematical nature, I did not wish to obtrude it upon the notice of chemical philosophers, but rather to wait till it was called for.—The resistance which any medium makes to the motion of a body depends upon the surface of that body, and is greater as the surface is greater, all other circumstances being the same. A ball of lead 1 inch in diameter meets with a certain resistance in falling through the air; but the same ball, being made into a thousand smaller ones of $\frac{1}{10}$ of an inch diameter, and falling with the same velocity, meets with 10 times the resistance it did before: because the force of gravity increases as the *cube* of the diameter of any particle, and the resistance only as the *square* of the diameter. Hence it appears that in order to increase the resistance of particles moving in any medium, it is only necessary to divide them, and that the resistance will be a maximum when the division is a maximum. We have only then to consider particles of lead falling through air by their own gravity, and we may have an idea of the resistance of

one gas entering another, *only the particles of lead must be conceived to be infinitely small,* if I may be allowed the expression. Here we shall find great resistance, and yet no one, I should suppose, will say that the air and the lead are mutually elastic.

Mr. Murray has lately edited a system of chemistry, in which he has given a very clear description of the phenomena of the atmosphere, and of other similar mixtures of elastic fluids. He has ably discussed the different theories that have been proposed on the subject, and given a perspicuous view of mine, which he thinks is ingenious, and calculated to explain several of the phenomena well, but, upon the whole, not equally satisfactory with that which he adopts. He does not object to the mechanism of my hypothesis in regard to the independent elasticity of the several gases entering into any mixture, but argues that the phenomena do not require so extraordinary a postulatum; and more particularly disapproves of the application of my theory to account for the evaporation.

The principal feature in Mr. Murray's theory, and which he thinks distinguishes it from mine, is "that between mixed gases, which are capable, under any circumstances of combining, an attraction must always be exerted."

Before we animadvert on these principles, it may be convenient to extend the first a little farther, and to adopt as a maxim, "that between the particles of *pure* gases, which are capable under any circumstances of combining, an attraction must always be exerted." This, Mr. Murray cannot certainly object to, in the case of steam, a pure elastic fluid, the particles of which are known in certain circumstances to combine. Nor will it be said that steam and a permanent gas are different; for he justly observes, "this distinction (between gases and vapours) is merely relative, and arises from the difference of temperature at which they are formed; the state with regard to each, while they exist in it, is precisely the same." Is steam then constituted of particles in which the attraction is so far exerted as to prevent their separation? No: they exhibit no traces of attraction, more than the like number of particles of oxygen do, when in the gaseous form. What then is the conclusion? It is this: *notwithstanding it must be allowed that all bodies, at all times, and in every situation, attract one another; yet in certain circumstances, they are likewise actuated by a repulsive power; the only efficient motive force is then the difference of these two powers.*

From the circumstance of gases mixing together without experiencing any sensible diminution of volume, the advocates for the agency of chemical affinity characterise it as a "slight action," and "a weak reciprocal action." So far I think they are consistent; but when we hear of this affinity being so far exerted as to prevent the separation of elastic particles, I do not conceive with what propriety it can be called weak. Suppose this affinity should be exercised in the case of steam of 212°; then the attraction becoming equal to the repulsion, the force which any one particle would exercise must be equal to the weight of a column of water of 4896 feet high.

It is somewhat remarkable that those gases which are known to combine occasionally, as azote and oxygen, and those which are never known to combine, as hydrogen and carbonic acid, should dissolve one another with equal facility; nay, these last exercise this solvent power with more effect than the former; for hydrogen can draw up carbonic acid from the bottom to the top of any vessel, notwithstanding the latter is 20 times the specific gravity of the former. One would have thought that a force of adhesion was more to be expected in the particles of steam than in a mixture of hydrogen and carbonic acid. But it is the business of those who adopt the theory of the mutual solution of gases to explain these difficulties.

In a mixture where are 8 particles of oxygen for 1 of hydrogen, it is demonstrable that the central distances of the particles of hydrogen are at a medium twice as great as those of oxygen. Now supposing the central distance of two adjacent particles of hydrogen to be denoted by 12, query, what is supposed to be the central distance of any one particle of hydrogen from that one particle, or those particles of oxygen with which it is connected by this weak chemical union? It would be well if those who understand and maintain the doctrine of chemical solution would represent how they conceive this to be; it would enable those who are desirous to learn, to obtain a clear idea of the system, and those who are dissatisfied with it, to point out its defects with more precision.

In discussing the doctrines of elastic fluids mixed with vapour, Mr. Murray seems disposed to question the accuracy of the fact that the quantity of vapour is the same in vacuo as in air, though he has not attempted to ascertain in which case it more abounds. This is certainly the touchstone of the mechanical and chemical theories; and I had thought that whoever admitted the truth of the fact must unavoidably adopt the mechanical theory. Berthollet however, convinced from his own experience that the fact was incontrovertible, attempts to reconcile it, inimical as it is, to the chemical theory; with what success it is left to others to judge. Mr. Murray joins with Berthollet in condemning as extravagant the position which I maintain, that if the atmosphere were annihilated, we should have little more aqueous vapour than at present exists in it. Upon which I shall only remark that if either of those gentlemen will calculate, or give a rough estimate upon their hypothesis, of the quantity of aqueous vapour that would be collected around the earth, on the said supposition, I will engage to discuss the subject with them more at large.

In 1802, Dr. Henry announced a very curious and important discovery, which was afterwards published in the *Philosophical Transactions;* namely, *that the quantity of any gas absorbed by water is increased in direct proportion to the pressure of the gas on the surface of the water.* Previously to this, I was engaged in an investigation of the quantity of carbonic acid in the atmosphere; it was matter of surprise to me that lime water should so readily manifest the presence of carbonic acid in the air, whilst pure water, by exposure for any length of time, gave not the least traces of that acid. I thought that length of time ought to compensate for weakness of affinity. In pursuing the subject I found that the quantity of

this acid taken up by water was greater or less in proportion to its greater or less density in the gaseous mixture, incumbent upon the surface, and therefore ceased to be surprised at water absorbing so insensible a portion from the atmosphere. I had not however entertained any suspicion that this law was generally applicable to the gases till Dr. Henry's discovery was announced. Immediately upon this, it struck me as essentially necessary, in ascertaining the quantity of any gas which a given volume of water will absorb, that we must be careful the gas is perfectly pure or unmixed with any other gas whatever; otherwise the maximum effect for any given pressure cannot be produced. This thought was suggested to Dr. Henry, and found to be correct; in consequence of which it became expedient to repeat some of his experiments relating to the quantity of gas absorbed under a given pressure. Upon due consideration of all these phenomena, Dr. Henry became convinced that there was no system of elastic fluids which gave so simple, easy and intelligible a solution of them as the one I adopt, namely, that each gas in any mixture exercises a distinct pressure, which continues the same if the other gases are withdrawn.

I shall now proceed to give my present views on the subject of mixed gases, which are somewhat different from what they were when the theory was announced, in consequence of the fresh lights which succeeding experience has diffused. In prosecuting my enquiries into the nature of elastic fluids, I soon perceived it was necessary, if possible, to ascertain whether the atoms or ultimate particles of the different gases are of the same size or volume in like circumstances of temperature and pressure. By the size or volume of an ultimate particle, I mean, in this place, the space it occupies in the state of a pure elastic fluid; in this sense the bulk of the particle signifies the bulk of the supposed impenetrable nucleus, together with that of its surrounding repulsive atmosphere of heat. At the time I formed the theory of mixed gases, I had a confused idea, as many have, I suppose, at this time, that the particles of elastic fluids are all of the same size; that a given volume of oxygenous gas contains just as many particles as the same volume of hydrogenous; or, if not, that we had no data from which the question could be solved. But from a train of reasoning I became convinced that different gases have *not* their particles of the same size; and that the following may be adopted as a maxim, till some reason appears to the contrary: namely,—

That every species of pure elastic fluid has its particles globular and all of a size; but that no two species agree in the size of their particles, the pressure and temperature being the same.

When we contemplate upon the disposition of the globular particles in a volume of pure elastic fluid, we perceive it must be analogous to that of a square pile of shot; the particles must be disposed into horizontal strata, each four particles forming a square: in a superior stratum, each particle rests upon four particles below, the points of its contact with all four being 45° above the horizontal plane, or that plane which passes through the centres of the four particles. On this account the pressure is steady and uniform throughout. But when a measure of one gas is pre-

sented to a measure of another in any vessel, we have then a surface of elastic globular particles of one size in contact with an equal surface of particles of another: in such case the points of contact of the heterogeneous particles must vary all the way from 40° to 90°; an intestine motion must arise from this inequality, and the particles of one kind be propelled amongst those of the other. The same cause which prevented the two elastic surfaces from maintaining an equilibrium will always subsist, the particles of one kind being from their size unable to apply properly to the other, so that no equilibrium can ever take place amongst the heterogeneous particles. The intestine motion must therefore continue till the particles arrive at the opposite surface of the vessel against any point of which they can rest with stability, and the equilibrium at length is acquired when each gas is uniformly diffused through the other. In the open atmosphere no equilibrium can take place in such case till the particles have ascended so far as to be restrained by their own weight; that is, till they constitute a distinct atmosphere.

It is remarkable that when two equal measures of different gases are thus diffused, and sustain an invaried pressure, as that of the atmosphere, the pressure upon each particle after the mixture is less than before. This points out the active principle of diffusion; for particles of fluids are always disposed to move to that situation where the pressure is least. Thus, in a mixture of equal measures of oxygen and hydrogen, the common pressure on each particle before mixture being denoted by 1, that after the mixture, when the gas becomes of half its density, will be denoted by $\sqrt[3]{\frac{1}{2}}=.794$.

This view of the constitution of mixed gases agrees with that which I have given before, in the two following particulars, which I consider as essential to every theory on the subject to give it plausibility.

1st. The diffusion of gases through each other is effected by means of the repulsion belonging to the homogenous particles; or to that principle which is always energetic to produce the dilatation of the gas.

2d. When any two or more mixed gases acquire an equilibrium, the elastic energy of each against the surface of the vessel or of any liquid is precisely the same as if it were the only gas present occupying the whole space, and all the rest were withdrawn.

In other respects I think the last view accords better with the phenomena.

Section 3. On the Constitution of Liquids, and the Mechanical Relations betwixt Liquids and Elastic Fluids

A liquid or inelastic fluid may be defined to be a body, the parts of which yield to a very small impression, and are easily moved one upon another. This definition may suffice for the consideration of liquids in an hydrostatical sense, but not in a chemical sense. Strictly speaking, there is no substance inelastic; but we commonly apply the word elastic to such fluids only as have the property of condensation in a very conspicuous de-

gree. Water is a liquid or inelastic fluid; but if it is compressed by a great force, it yields a little, and again recovers its original bulk when the pressure is removed. We are indebted to Mr. Canton for a set of experiments by which the compressibility of several liquids is demonstrated. Water, he found, lost about $\frac{1}{21740}$th part of its bulk by the pressure of the atmosphere.

When we consider the origin of water from steam, we have no reason to wonder at its compressibility, and that in a very small degree; it would be wonderful if water had not this quality. The force of steam at 212° is equal to the pressure of the atmosphere; what a prodigious force must it have when condensed 15 or 18 hundred times? The truth is, water, and, by analogy, other liquids, must be considered as bodies, under the control of two most powerful and energetic agents, attraction and repulsion, between which there is an equilibrium. If any compressing force is applied, it yields, indeed, but in such a manner as a strong spring would yield when wound up almost to the highest pitch. When we attempt to separate one portion of liquid from another, the case is different: here the attraction is the antagonist force, and that being balanced by the repulsion of the heat, a moderate force is capable of producing the separation. But even here we perceive the attractive force to prevail, there being a manifest cohesion of the particles. Whence does this arise? It should seem that when two particles of steam coalesce to form water, they take their station so as to effect a perfect equilibrium between the two opposite powers; but if any foreign force intervene, so as to separate the two molecules an evanescent space, the repulsion decreases faster than the attraction, and consequently this last acquires a superiority or excess, which the foreign force has to overcome. If this were not the case, why do they at first, or upon the formation of water, pass from the greater to the less distance?

With regard to the collocation and arrangement of particles in an aggregate of water or any other liquid, I have already observed that this is not, in all probability, the same as in air. It seems highly improbable from the phenomena of the expansion of liquids by heat. The law of expansion is unaccountable for, if we confine liquids to one and the same arrangement of their ultimate particles in all temperatures; for we cannot avoid concluding, if that were the case, the expansion would go on in a progressive way with the heat, like as in air; and there would be no such thing observed as a point of temperature at which the expansion was stationary.

RECIPROCAL PRESSURE OF LIQUIDS AND ELASTIC FLUIDS

When an elastic fluid is confined by a vessel of certain materials, such as wood, earthenware, &c., it is found slowly to communicate with the external air, to give and receive successively, till a complete intermixture takes place. There is no doubt but this is occasioned by those vessels being porous, so as to transmit the fluids. Other vessels, as those of metal, glass, &c., confine air most completely. These therefore cannot be porous;

or rather, their pores are too small to admit of the passage of air. I believe no sort of vessel has yet been found to transmit one gas and confine another; such a one is a desideratum in practical chemistry. All the gases appear to be completely porous, as might be expected, and therefore operate very temporarily in confining each other. How are liquids in this respect? Do they resemble glass, or earthenware, or gases, in regard to their power of confining elastic fluids? Do they treat all gases alike, or do they confine some and transmit others? These are important questions: they are not to be answered in a moment. We must patiently examine the facts.

Before we can proceed, it will be necessary to lay down a rule, if possible, by which to distinguish the *chemical* from the *mechanical* action of a liquid upon an elastic fluid. I think the following cannot well be objected to: *When an elastic fluid is kept in contact with a liquid, if any change is perceived, either in the elasticity or any other property of the elastic fluid, so far the mutual action must be pronounced* CHEMICAL: *but if* NO *change is perceived, either in the elasticity or any other property of the elastic fluid, then the mutual action of the two must be pronounced wholly* MECHANICAL.

If a quantity of lime be kept in water and agitated, upon standing a sufficient time, the lime falls down, and leaves the water transparent: but the water takes a small portion of the lime which it permanently retains, contrary to the Laws of specific gravity. Why? Because that portion of lime is dissolved by the water. If a quantity of air be put to water and agitated, upon standing a sufficient time, the air rises up to the surface of the water and leaves it transparent; but the water permanently retains a portion of air, contrary to the Laws of specific gravity. Why? Because that small portion of air is dissolved by the water. So far the two explanations are equally satisfactory. But if we place the two portions of water under the receiver of an air pump, and exhaust the incumbent air, the whole portion of air absorbed by the water ascends, and is drawn out of the receiver; whereas the lime remains still in solution as before. If now the question be repeated, why is the air retained in the water? The answer must be, because there is an elastic force on the surface of the water which holds it in. The water appears passive in the business. But, perhaps, the pressure on the surface of the water may have some effect upon its affinity for air, and none on that for lime? Let the air be drawn off from the surfaces of the two portions of water, and another species induced without alleviating the pressure. The lime water remains unchanged; the air escapes from the other much the same as in vacuo. The question of the relation of water to air appears by this fact to be still more difficult; at first the air seemed to be retained by the attraction of the water; in the second case, the water seemed indifferent; in the third, it appears as if repulsive to the air; yet in all three, it is the same air that has to act on the same water. From these facts, there seems reason then for maintaining three opinions on the subject of the mutual action of air and water; namely, that water attracts air, that water does not attract it, and that water repels air. One of these must be true; but we must not decide

hastily. Dr. Priestley once imagined that the clay of a porous earthen retort, when red hot, "destroys for a time the aërial form of whatever air is exposed to the outside of it; which aërial form it recovers, after it has been transmitted in combination from one part of the clay to another, till it has reached the inside of the retort." But he soon discarded so extravagant an opinion.

From the recent experiments of Dr. Henry, with those of my own, there appears reason to conclude that a given volume of water absorbs the following parts of its bulk of the several gases.

Bulk of gas absorbed.

1	= 1	Carbonic acid
1	= 1	Sulphuretted hydrogen
1	= 1	Nitrous oxide
⅛	= .125	Olefiant gas
1/27	= .037	Oxygenous gas
1/27	= .037	Nitrous gas
1/27	= .037	Carburetted hydrogen
1/27	= .037	Carbonic oxide?
1/64	= .0156	Azotic gas
1/64	= .0156	Hydrogenous gas
1/64	= .0156	Carbonic oxide?

These fractions are the cubes of 1/1, ½, ⅓, ¼, &c. This shews the distances of the gaseous particles in the water to be always same multiple of the distances without.

In a mixture of two or more gases, the rule holds the same as when the gases are alone; that is, the quantity of each absorbed is the same as if it was the only gas present.

As the quantity of any gas in a given volume is subject to variation from pressure and temperature, it is natural to enquire whether any change is induced in the absorption of these circumstances; the experiments of Dr. Henry have decided this point, by ascertaining that if the exterior gas is condensed or rarefied in any degree, the gas absorbed is condensed or rarefied in the same degree; so that the proportions absorbed given above are absolute.

One remarkable fact, which has been hinted at, is that no one gas is capable of retaining another in water; it escapes, not indeed instantly, like as in a vacuum, but gradually, like as carbonic acid escapes into the atmosphere from the bottom of a cavity communicating with it.

It remains now to decide whether the relation between water and the above-mentioned gases is of a *chemical* or *mechanical* nature. From the facts just stated, it appears evident that the elasticity of carbonic acid and the other two gases of the first class is not at all affected by the water. It remains exactly of the same energy whether the water is present or absent. All the other properties of those gases continue just the same, as far as I know, whether they are alone or blended with water: we must therefore,

ceive, if we abide by the Law just laid down, pronounce the mutual
n between these gases and water to be *mechanical.*
In the other gases it is very remarkable their density within the water
ld be such as to require the distance of the particles to be just 2, 3 or
ıes what it is without. In olefiant gas, the distance of the particles
in is just twice that without, as is inferred from the density being
n oxygenous gas, &c., the distance is 3 times as great, and in hydrog-
s, &c., 4 times. This is certainly curious, and deserves further investi-
n; but at present we have only to decide whether the general phe-
ena denote the relation to be of a chemical or mechanical nature. In
ıse whatever does it appear that the elasticity of any of these gases is
ed; if water takes $\frac{1}{27}$ of its bulk of any gas, the gas so absorbed
s $\frac{1}{27}$ of the elasticity that the exterior gas does, and of course it
ıes from the water when the pressure is withdrawn from its surface,
hen a foreign one is induced, against which it is not a proper match.
ır as is known too, all the other properties of the gases continue the
; thus, if water containing oxygenous gas be admitted to nitrous gas,
ınion of the two gases is certain; after which the water takes up $\frac{1}{27}$
bulk of nitrous gas, as it would have done, if this circumstance had
ıccurred. It seems clear then that the relation is a *mechanical* one.
Carbonic acid gas then presses upon water in the first instance with
hole force; in a short time it partly enters the water, and then the
ion of the part entered contributes to support the incumbent atmos-
e. Finally, the gas gets completely diffused through the water, so as
of the same density within as without; the gas within the water then
es on the containing vessel only, and reacts upon the incumbent gas.
water then sustains no pressure either from the gas within or with-
In olefiant gas the surface of the water supports $\frac{7}{8}$ of the pressure,
ygenous, &c., $\frac{26}{27}$, and in hydrogenous, &c., $\frac{63}{64}$.
When any gas is confined in a vessel over water in the pneumatic
gh, so as to communicate with the atmosphere through the medium
ater, that gas must constantly be filtring through the water into the
sphere, whilst the atmospheric air is filtring through the water the
ary way, to supply its place in the vessel; so that in due time the air
e vessel becomes atmospheric, as various chemists have experienced.
er in this respect is like an earthenware retort: it admits the gases to
oth ways at the same time.
It is not easy to assign a reason why water should be so permeable to
onic acid, &c., and not to the other gases; and why there should be
e differences observable in the others. The densities $\frac{1}{8}$, $\frac{1}{27}$ and $\frac{1}{64}$
most evidently a reference to a mechanical origin, but none whatever
chemical one. No mechanical equilibrium could take place if the den-
s of the gases within were not regulated by this law; but why the gases
ld not all agree in some one of these forms, I do not see any reason.
Upon the whole it appears that water, like earthenware, is incapable
rming a perfect barrier to any kind of air; but it differs from earthen-
in one respect; the last is alike permeable to all the gases, but water

is much more permeable to some gases than to others. Other liquids have not been sufficiently examined in this respect.

Section 4. On the Constitution of Solids

A solid body is one, the particles of which are in a state of equilibrium betwixt two great powers, attraction and repulsion, but in such a manner that no change can be made in their distances without considerable force.

Notwithstanding the *hardness* of solid bodies, or the difficulty of moving the particles one amongst another, there are several that admit of such motion without fracture, by the application of proper force, especially if assisted by heat. The ductility and malleability of the metals need only to be mentioned. It should seem the particles glide along each other's surface, somewhat like a piece of polished iron at the end of a magnet, without being at all weakened in their cohesion. The absolute force of cohesion, which constitutes the *strength* of bodies, is an enquiry of great practical importance. It has been found by experiment that wires of the several metals beneath, being each $\frac{1}{10}$ of an inch in diameter, were just broken by the annexed weights.

Lead	29¼	Pounds.
Tin	49¼	
Copper	299¼	
Brass	360	
Silver	370	
Iron	450	
Gold	500	

A piece of good oak, an inch square and a yard long, will just bear in the middle 330 lbs. But such a piece of wood should not in practice be trusted, for any length of time, with above ⅓ or ¼ of that weight. Iron is about 10 times as strong as oak, of the same dimensions.

One would be apt to suppose that *strength* and *hardness* ought to be found proportionate to each other; but this is not the case. Glass is harder than iron, yet the latter is much the stronger of the two.

Crystallization exhibits to us the effects of the natural arrangement of the ultimate particles of various compound bodies; but we are scarcely yet sufficiently acquainted with chemical synthesis and analysis to understand the rationale of this process. The rhomboidal form may arise from the proper position of 4, 6, 8, or 9 globular particles, the cubic form from 8 particles, the triangular form from 3, 6 or 10 particles, the hexahedral prism from 7 particles, &c. Perhaps, in due time, we may be enabled to ascertain the number and order of elementary particles, constituting any given compound element, and from that determine the figure which it will prefer on crystallization, and *vice versa;* but it seems premature to form any theory on this subject till we have discovered from other principles the number and order of the primary elements which combine to

form some of the compound elements of most frequent occurrence; the method for which we shall endeavour to point out in the ensuing chapter.

II. ON CHEMICAL SYNTHESIS

WHEN any body exists in the elastic state, its ultimate particles are separated from each other to a much greater distance than in any other state; each particle occupies the centre of a comparatively large sphere, and supports its dignity by keeping all the rest, which by their gravity, or otherwise, are disposed to encroach up it, at a respectful distance. When we attempt to conceive the *number* of particles in an atmosphere, it is somewhat like attempting to conceive the number of stars in the universe; we are confounded with the thought. But if we limit the subject, by taking a given volume of any gas, we seem persuaded that, let the divisions be ever so minute, the number of particles must be finite; just as in a given space of the universe, the number of stars and planets cannot be infinite.

Chemical analysis and synthesis go no farther than to the separation of particles one from another, and to their reunion. No new creation or destruction of matter is within the reach of chemical agency. We might as well attempt to introduce a new planet into the solar system, or to annihilate one already in existence, as to create or destroy a particle of hydrogen. All the changes we can produce consist in separating particles that are in a state of cohesion or combination, and joining those that were previously at a distance.

In all chemical investigations, it has justly been considered an important object to ascertain the relative *weights* of the simples which constitute a compound. But unfortunately the enquiry has terminated here; whereas from the relative weights in the mass, the relative weights of the ultimate particles or atoms of the bodies might have been inferred, from which their number and weight in various other compounds would appear, in order to assist and to guide future investigations, and to correct their results. Now it is one great object of this work to shew the importance and advantage of ascertaining *the relative weights of the ultimate particles, both of simple and compound bodies, the number of simple elementary particles which constitute one compound particle, and the number of less compound particles which enter into the formation of one more compound particle.*

If there are two bodies, A and B, which are disposed to combine, the following is the order in which the combinations may take place, beginning with the most simple: namely,

1 atom of A + 1 atom of B = 1 atom of C, binary.
1 atom of A + 2 atoms of B = 1 atom of D, ternary.
2 atoms of A + 1 atom of B = 1 atom of E, ternary.
1 atom of A + 3 atoms of B = 1 atom of F, quaternary.
3 atoms of A + 1 atom of B = 1 atom of G, quaternary.
&c. &c.

The following general rules may be adopted as guides in all our investigations respecting chemical synthesis.

1st. When only one combination of two bodies can be obtained, it must be presumed to be a *binary* one, unless some cause appear to the contrary.

2d. When two combinations are observed, they must be presumed to be a *binary* and a *ternary*.

3d. When three combinations are obtained, we may expect one to be a *binary*, and the other two *ternary*.

4th. When four combinations are observed, we should expect one *binary*, two *ternary*, and one *quaternary*, &c.

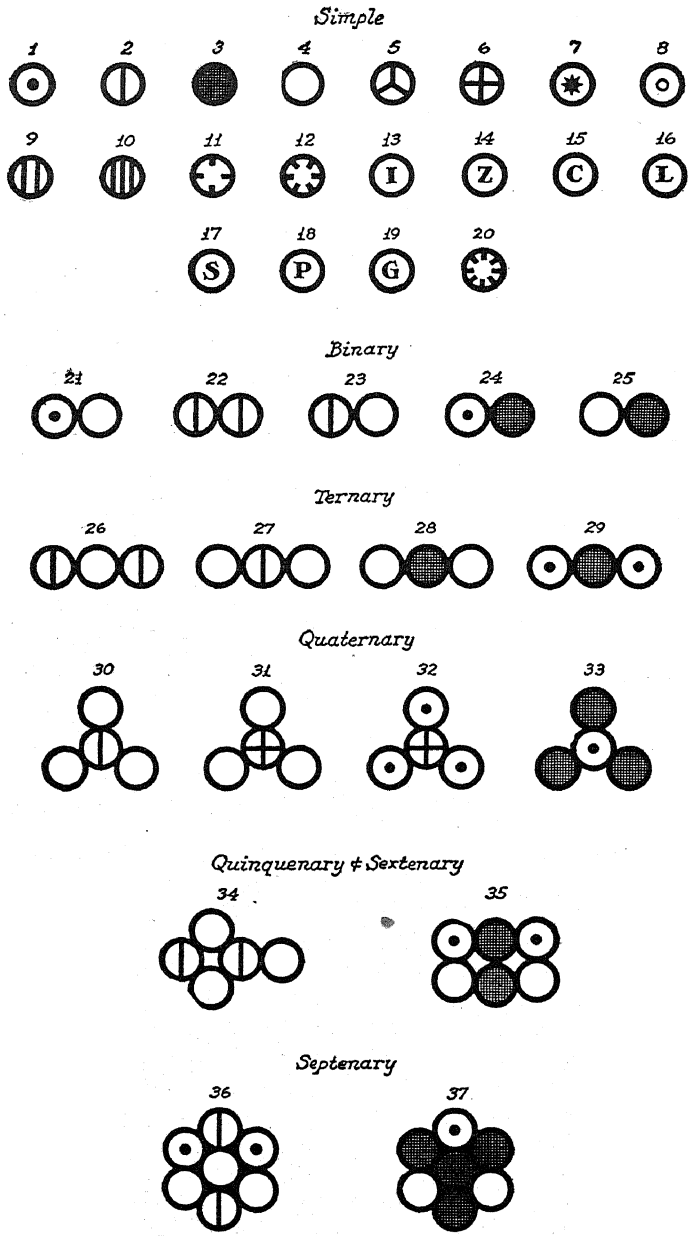
5th. A *binary* compound should always be specifically heavier than the mere mixture of its two ingredients.

6th. A *ternary* compound should be specifically heavier than the mixture of a binary and a simple, which would, if combined, constitute it; &c.

7th. The above rules and observations equally apply when two bodies, such as C and D, D and E, &c., are combined.

From the application of these rules to the chemical facts already well ascertained, we deduce the following conclusions: 1st. That water is a binary compound of hydrogen and oxygen, and the relative weights of the two elementary atoms are as 1:7, nearly; 2d. That ammonia is a binary compound of hydrogen and azote, and the relative weights of the two atoms are as 1:5, nearly; 3d. That nitrous gas is a binary compound of azote and oxygen, the atoms of which weigh 5 and 7 respectively; that nitric acid is a binary or ternary compound according as it is derived, and consists of one atom of azote and two of oxygen, together weighing 19; that nitrous oxide is a compound similar to nitric acid, and consists of one atom of oxygen and two of azote, weighing 17; that nitrous acid is a binary compound of nitric acid and nitrous gas, weighing 31; that oxynitric acid is a binary compound of nitric acid and oxygen, weighing 26; 4th. That carbonic oxide is a binary compound, consisting of one atom of charcoal and one of oxygen, together weighing nearly 12; that carbonic acid is a ternary compound (but sometimes binary), consisting of one atom of charcoal and two of oxygen, weighing 19; &c., &c. In all these cases the weights are expressed in atoms of hydrogen, each of which is denoted by unity.

From the novelty as well as importance of the ideas suggested in this chapter, it is deemed expedient to give a plate exhibiting the mode of combination in some of the more simple cases. The elements or atoms of such bodies as are conceived at present to be simple are denoted by a small circle, with some distinctive mark; and the combinations consist in the juxtaposition of two or more of these; when three or more particles of elastic fluids are combined together in one, it is to be supposed that the particles of the same kind repel each other, and therefore take their stations accordingly.


Simple
1
2
3
4
5
6
7
8
9
10
11
12
13
I
14
Z
15
C
16
L
17
S
18
P
19
G
20
Binary
21
22
23
24
25
Ternary
26
27
28
29
Quaternary
30
31
32
33
Quinquenary & Sextenary
34
35
Septenary
36
37

EXPLANATION OF PLATE

This plate contains the arbitrary marks or signs chosen to represent the several chemical elements or ultimate particles.

1. Hydrog. its rel. weight ...	1	11. Strontites	46
2. Azote	5	12. Barytes	68
3. Carbone or charcoal	5	13. Iron	38
4. Oxygen	7	14. Zinc	56
5. Phosphorus	9	15. Copper	56
6. Sulphur	13	16. Lead	95
7. Magnesia	20	17. Silver	100
8. Lime	23	18. Platina	100
9. Soda	28	19. Gold	140
10. Potash	42	20. Mercury	167

21. An atom of water or steam, composed of 1 of oxygen and 1 of hydrogen, retained in physical contact by a strong affinity, and supposed to be surrounded by a common atmosphere of heat; its relative weight = ... 8
22. An atom of ammonia, composed of 1 of azote and 1 of hydrogen 6
23. An atom of nitrous gas, composed of 1 of azote and 1 of oxygen 12
24. An atom of olefiant gas, composed of 1 of carbone and 1 of hydrogen 6
25. An atom of carbonic oxide composed of 1 of carbone and 1 of oxygen 12
26. An atom of nitrous oxide, 2 azote+1 oxygen 17
27. An atom of nitric acid, 1 azote+2 oxygen 19
28. An atom of carbonic acid, 1 carbone+2 oxygen 19
29. An atom of carburetted hydrogen, 1 carbone+2 hydrogen 7
30. An atom of oxynitric acid, 1 azote+3 oxygen 26
31. An atom of sulphuric acid, 1 sulphur+3 oxygen 34
32. An atom of sulphuretted hydrogen, 1 sulphur+3 hydrogen 16
33. An atom of alcohol, 3 carbone+1 hydrogen 16
34. An atom of nitrous acid, 1 nitric acid+1 nitrous gas 31
35. An atom of acetous acid, 2 carbone+2 water 26
36. An atom of nitrate of ammonia, 1 nitric acid+1 ammonia+1 water 33
37. An atom of sugar, 1 alcohol+1 carbonic acid 35

Enough has been given to shew the method; it will be quite unnecessary to devise characters and combinations of them to exhibit to view in this way all the subjects that come under investigation; nor is it necessary to insist upon the accuracy of all these compounds, both in number and weight; the principle will be entered into more particularly hereafter, as far as respects the individual results. It is not to be understood that all those particles marked as simple substances are necessarily such by the theory;

they are only necessarily of such weights. Soda and potash, such as they are found in combination with acids, are 28 and 42 respectively in weight; but according to Mr. Davy's very important discoveries, they are metallic oxides; the former then must be considered as composed of an atom of metal, 21, and one of oxygen, 7; and the latter, of an atom of metal, 35, and one of oxygen, 7. Or, soda contains 75 per cent metal and 25 oxygen; potash, 83.3 metal and 16.7 oxygen. It is particularly remarkable that according to the above-mentioned gentleman's essay on the Decomposition and Composition of the fixed alkalies in the *Philosophical Transactions* (a copy of which essay he has just favoured me with), it appears that "the largest quantity of oxygen indicated by these experiments was, for potash 17, and for soda, 26 parts in 100, and the smallest 13 and 19."

PRINCIPLES OF GEOLOGY

by

CHARLES LYELL

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CHARLES LYELL

1797–1875

CHARLES LYELL, a devoted student of Dante and a good botanist, had married the daughter of Thomas Smith of Yorkshire and had settled in Forfarshire in Scotland. His eldest child—the first of ten—was born there in 1797 and named Charles. A year later the family moved to Bartley Lodge in the New Forest, in the extreme south of England. The boy had his early training in private schools and at nineteen entered Exeter College, Oxford. He took a very good degree in 1819 (second class in Classical Honors) and an M.A. in 1821. Though he had early shown the traits of the amateur naturalist, he did not follow his father into botany, but assiduously studied entomology. Before he had finished his undergraduate work at Oxford, he had been attracted to geology by the lectures of Dr. William Buckland. During a holiday he had noted the evidence of recent changes in the coast line near Norwich; he had used another holiday for a tour of the central Grampians (in Scotland), and others for tours of the western isle of Mull, and of Europe across the Juras and Alps to Florence. His family afforded Lyell such expeditions. His choice took him where he could foster his growing interest in geology.

When he settled in London in 1819 to read law—to which he was faithful for ten years—at Lincoln's Inn, he naturally joined the Linnaean Society and the Geological Society. Four years later he became secretary of the Geological Society, and in the next year read to its members a paper, "On a Recent Formation of Freshwater Limestone in Forfarshire." In this paper he emphasized the resemblance between deposits in ancient and modern lakes. He was already moving toward the generalization which his subsequent work in geology is based upon: that the processes of the past must be judged by those now in progress.

Another paper resulting from his tour of the Grampians,

"On a Dike of Serpentine in the County of Forfar," appeared in the Edinburgh *Journal of Science* in 1825, Lyell's first published paper. The following year he was elected a Fellow of the Royal Society. Now he collaborated with Dr. Mandell in some studies of the cretaceous beds of southeast England, and he began to meet the first of those great scientists who were to be his lifelong friends: Cuvier, Laplace, Arago, Humboldt.

An article by Lyell in the *Quarterly Review* in 1827 criticizes those who measure facts not by observation but by appeal to the literal text of the Holy Scriptures. Lyell did not intend an attack upon revealed religion, for he was all his life a religious man. Convinced of the validity of the methods of the natural scientist, he saw that scientists' results could not have their appropriate influence so long as misguided, even ignorant, criticism misinterpreted them. Once convinced, he spoke his mind. About the same time he reached another conclusion which entered into his later theorizing: that negative evidence can never be conclusive. Already, in distinction from his predecessors in whose times paleontology was an infant science, he was ardently following the discoveries and conclusions of the paleontologists. At the moment he was concerned with the claims that birds and mammals were created late in the history of the world, inasmuch as no evidence of them had been discovered in the earlier geologic strata. He insisted that the evidence, being negative, proved nothing; subsequent discoveries proved him at least partly right.

In 1828, Lyell journeyed in Auvergne, went on to Padua, and then, despite some physical hazards involved, proceeded to Sicily. There he saw evidence of recent mountain building and of land elevation. He was now confirmed in his earlier idea that existing causes have always been the efficient causes of geological change. On the same trip, in concurrence with the French conchologist Deshayes, he observed that the relative ages of the deposits in the more recent rocks could be determined by the proportion among their molluscan fossils of extinct to still extant species. He therefore classified the strata of the tertiary rocks in the order of their age as Eocene (dawn of recent), Miocene (less recent), and Pliocene (more recent). This classification he presented to the public in the third volume of the *Principles of Geology* in 1833.

The first volume of the *Principles* had appeared in 1830; the completed work did not appear until 1834. It was revolutionary. In 1830 the prevailing method among geologists of explaining the physical characteristics of the earth's crust was to adumbrate numerous cataclysms in the course of its history. Some geologists believed that a series of fiery, volcanic

crises had occurred at various times. Others, like Buckland, talked of recurrent deluges. Still others held that there had been catastrophes of both kinds. To the catastrophic school of thought the *Principles* gave a mortal wound. Appealing to paleontological evidence and logically marshaling his tremendous data, Lyell proved—what James Hutton had earlier suggested—that the phenomena could all be explained on the basis of still acting causes as the constant efficient causes. Simultaneously he enormously extended the concept of geologic time. Whereas his predecessors and even his early contemporaries confined the history of the world to a few thousand years, he showed that in this history time must be reckoned in eons, in millions of years.

In 1831, Lyell had been named professor of geology in King's College, London. But he never cared for his professorial duties; he actually gave only two courses of lectures (1832, 1833) at the college. In 1832 he delivered seven lectures at the Royal Institution. In 1834 the Royal Society voted him one of its two gold medals for the *Principles*.

Meantime, in 1832, Lyell had married Mary Horner, daughter of a man influential in the early history of the Geological Society. She became the constant companion of his many expeditions, and because of his increasing myopia she served him as amanuensis. They both enjoyed social life, and their house in Harley Street was a regular meeting place for a group of friends who in their letters have made it famous—Hallam, Dean Milman, Rogers, Darwin. They both enjoyed traveling, and they journeyed to the United States in 1841, in 1845, in 1852, and again in 1853; to the Canary Islands in 1854; frequently to various parts of Europe. When he was more than sixty Lyell went once more to Sicily to climb Etna and to study the formation of its lava beds.

Two of the trips to the United States were undertaken partly that Lyell might deliver the Lowell Institute lectures in Boston; another, that he might serve as British Commissioner to the New York International Exhibition of 1853. Every journey resulted in a paper—such as the study of changes in the level of the Baltic in recent times (1835)—or a book—such as *Travels in North America with Geological Observations,* 1845. Besides, Lyell wrote other papers indefatigably—seventy-six of them for the Royal Society—and so constantly revised his *Principles,* bringing it up to date, that no two of the eleven editions which appeared in his lifetime are identical. In 1838 he published an elaborated portion of the original *Principles* as a separate work, *Elements of Geology,* a descriptive textbook. In 1863 he completed his *Antiquity of Man,*

and in 1871 published *The Students' Elements of Geology,* long the one standard text for beginners in geology.

Lyell served the Geological Society as president in 1835 and 1836, and again in 1849 and 1850. Queen Victoria knighted him in 1848 and raised him to a baronetcy in 1864. Oxford granted him the degree of D.C.L. in 1854. In 1862 the Institute of France elected him a foreign correspondent, and in 1864 the British Association made him its president. He was everywhere recognized in his latest years as one of England's foremost scientists. When he died in 1875, he was buried in Westminster Abbey among his peers.

Lyell's friends greatly mourned his death; so did the learned world which knew his great and honest abilities. He had early refused to accept Darwin's new theories, friends though the two men were. Yet it was he who procured the publication of Darwin's and Wallace's first studies; and when the accumulated evidence persuaded him of the evolutionary doctrine, no one more wholeheartedly gave it support. The change in view was characteristic of the man. Greatly learned, he never became pedantic; tenacious in his beliefs, he was always open to reason.

PRINCIPLES OF GEOLOGY

I. Geology Defined

GEOLOGY is the science which investigates the successive changes that have taken place in the organic and inorganic kingdoms of nature; it inquires into the causes of these changes, and the influence which they have exerted in modifying the surface and external structure of our planet.

Geology is intimately related to almost all the physical sciences, as history is to the moral. An historian should, if possible, be at once profoundly acquainted with ethics, politics, jurisprudence, the military art, theology; in a word, with all branches of knowledge by which any insight into human affairs, or into the moral and intellectual nature of man, can be obtained. It would be no less desirable that a geologist should be well versed in chemistry, natural philosophy, mineralogy, zoology, comparative anatomy, botany; in short, in every science relating to organic and inorganic nature. With these accomplishments, the historian and geologist would rarely fail to draw correct and philosophical conclusions from the various monuments transmitted to them of former occurrences. They would know to what combination of causes analogous effects were referable, and they would often be enabled to supply, by inference, information concerning many events unrecorded in the defective archives of former ages. But as such extensive acquisitions are scarcely within the reach of any individual, it is necessary that men who have devoted their lives to different departments should unite their efforts; and as the historian receives assistance from the antiquary, and from those who have cultivated different branches of moral and political science, so the geologist should avail himself of the aid of many naturalists, and particularly of those who have studied the fossil remains of lost species of animals and plants.

The analogy, however, of the monuments consulted in geology, and those available in history, extends no farther than to one class of historical monuments—those which may be said to be *undesignedly* commemorative of former events. The canoes, for example, and stone hatchets found in our peat bogs, afford an insight into the rude arts and manners of the earliest inhabitants of our island; the buried coin fixes the date of the reign of some Roman emperor; the ancient encampment indicates the districts once occupied by invading armies, and the former method of constructing military defences: the Egyptian mummies throw light on the art of embalming, the rites of sepulture, or the average stature of the human

race in ancient Egypt. This class of memorials yields to no other in authenticity, but it constitutes a small part only of the resources on which the historian relies, whereas in geology it forms the only kind of evidence which is at our command. For this reason we must not expect to obtain a full and connected account of any series of events beyond the reach of history. But the testimony of geological monuments, if frequently imperfect, possesses at least the advantage of being free from all intentional misrepresentation. We may be deceived in the inferences which we draw, in the same manner as we often mistake the nature and import of phenomena observed in the daily course of nature; but our liability to err is confined to the interpretation, and, if this be correct, our information is certain.

II. Prejudices Which Have Retarded the Progress of Geology

IF WE REFLECT on the history of the progress of geology, we perceive that there have been great fluctuations of opinion respecting the nature of the causes to which all former changes of the earth's surface are referable. The first observers conceived the monuments which the geologist endeavours to decipher to relate to an original state of the earth, or to a period when there were causes in activity, distinct, in kind and degree, from those now constituting the economy of nature. These views were gradually modified, and some of them entirely abandoned in proportion as observations were multiplied, and the signs of former mutations more skilfully interpreted. Many appearances, which had for a long time been regarded as indicating mysterious and extraordinary agency, were finally recognized as the necessary result of the laws now governing the material world; and the discovery of this unlooked-for conformity has at length induced some philosophers to infer that, during the ages contemplated in geology, there has never been any interruption to the agency of the same uniform laws of change. The same assemblage of general causes, they conceive, may have been sufficient to produce, by their various combinations, the endless diversity of effects, of which the shell of the earth has preserved the memorials; and, consistently with these principles, the recurrence of analogous changes is expected by them in time to come.

Prepossessions in regard to the duration of past time.—Now the reader may easily satisfy himself that, however undeviating the course of nature may have been from the earliest epochs, it was impossible for the first cultivators of geology to come to such a conclusion, so long as they were under a delusion as to the age of the world, and the date of the first creation of animate beings. However fantastical some theories of the sixteenth century may now appear to us—however unworthy of men of great

talent and sound judgment—we may rest assured that, if the same misconception now prevailed in regard to the memorials of human transactions, it would give rise to a similar train of absurdities. Let us imagine, for example, that Champollion, and the French and Tuscan literati lately engaged in exploring the antiquities of Egypt, had visited that country with a firm belief that the banks of the Nile were never peopled by the human race before the beginning of the nineteenth century, and that their faith in this dogma was as difficult to shake as the opinion of our ancestors, that the earth was never the abode of living beings until the creation of the present continents, and of the species now existing—it is easy to perceive what extravagant systems they would frame, while under the influence of this delusion, to account for the monuments discovered in Egypt. The sight of the pyramids, obelisks, colossal statues, and ruined temples, would fill them with such astonishment, that for a time they would be as men spellbound—wholly incapable of reasoning with sobriety. They might incline at first to refer the construction of such stupendous works to some superhuman powers of a primeval world. A system might be invented resembling that so gravely advanced by Manetho, who relates that a dynasty of gods originally ruled in Egypt, of whom Vulcan, the first monarch, reigned nine thousand years; after whom came Hercules and other demigods, who were at last succeeded by human kings.

These speculations, if advocated by eloquent writers, would not fail to attract many zealous votaries, for they would relieve men from the painful necessity of renouncing preconceived opinions. But when one generation had passed away, and another, not compromised to the support of antiquated dogmas, had succeeded, they would review the evidence afforded by mummies more impartially, and would no longer controvert the preliminary question, that human beings had lived in Egypt before the nineteenth century: so that when a hundred years perhaps had been lost, the industry and talents of the philosopher would be at last directed to the elucidation of points of real historical importance.

But the above arguments are aimed against one only of many prejudices with which the earlier geologists had to contend. Even when they conceded that the earth had been peopled with animate beings at an earlier period than was at first supposed, they had no conception that the quantity of time bore so great a proportion to the historical era as is now generally conceded. How fatal every error as to the quantity of time must prove to the introduction of rational views concerning the state of things in former ages may be conceived by supposing the annals of the civil and military transactions of a great nation to be perused under the impression that they occurred in a period of one hundred instead of two thousand years. Such a portion of history would immediately assume the air of a romance; the events would seem devoid of credibility, and inconsistent with the present course of human affairs. A crowd of incidents would follow each other in thick succession. Armies and fleets would appear to be assembled only to be destroyed, and cities built merely to fall in ruins. There would be the most violent transitions from foreign or intestine war

to periods of profound peace, and the works effected during the years of disorder or tranquillity would appear alike superhuman in magnitude.

We should be warranted in ascribing the erection of the great pyramid to superhuman power, if we were convinced that it was raised in one day; and if we imagine, in the same manner, a continent or mountain chain to have been elevated, during an equally small fraction of the time which was really occupied in upheaving it, we might then be justified in inferring that the subterranean movements were once far more energetic than in our own times. We know that during one earthquake the coast of Chili may be raised for a hundred miles to the average height of about three feet. A repetition of two thousand shocks, of equal violence, might produce a mountain chain one hundred miles long and six thousand feet high. Now, should one or two only of these convulsions happen in a century, it would be consistent with the order of events experienced by the Chilians from the earliest times: but if the whole of them were to occur in the next hundred years, the entire district must be depopulated, scarcely any animals or plants could survive, and the surface would be one confused heap of ruin and desolation.

Prejudices arising from our peculiar position as inhabitants of the land.—The sources of prejudice hitherto considered may be deemed peculiar for the most part to the infancy of the science, but others are common to the first cultivators of geology and to ourselves, and are all singularly calculated to produce the same deception and to strengthen our belief that the course of nature in the earlier ages differed widely from that now established.

The first and greatest difficulty consists in an habitual unconsciousness that our position as observers is essentially unfavourable, when we endeavour to estimate the nature and magnitude of the changes now in progress. In consequence of our inattention to this subject, we are liable to serious mistakes in contrasting the present with former states of the globe. As dwellers on the land, we inhabit about a fourth part of the surface; and that portion is almost exclusively a theatre of decay, and not of reproduction. We know, indeed, that new deposits are annually formed in seas and lakes, and that every year some new igneous rocks are produced in the bowels of the earth, but we cannot watch the progress of their formation; and as they are only present to our minds by the aid of reflection, it requires an effort both of the reason and the imagination to appreciate duly their importance. It is, therefore, not surprising that we estimate very imperfectly the result of operations thus invisible to us; and that, when analogous results of former epochs are presented to our inspection, we cannot immediately recognise the analogy. He who has observed the quarrying of stone from a rock, and has seen it shipped for some distant port, and then endeavours to conceive what kind of edifice will be raised by the materials, is in the same predicament as a geologist, who, while he is confined to the land, sees the decomposition of rocks, and the transportation of matter by rivers to the sea, and then endeavours to picture to himself the new strata which Nature is building beneath the waters.

Prejudices arising from our not seeing subterranean changes.—Nor is his position less unfavourable when, beholding a volcanic eruption, he tries to conceive what changes the column of lava has produced, in its passage upwards, on the intersected strata; or what form the melted matter may assume at great depths on cooling; or what may be the extent of the subterranean rivers and reservoirs of liquid matter far beneath the surface. It should, therefore, be remembered that the task imposed on those who study the earth's history requires no ordinary share of discretion; for we are precluded from collating the corresponding parts of the system of things as it exists now and as it existed at former periods. If we were inhabitants of another element—if the great ocean were our domain, instead of the narrow limits of the land—our difficulties would be considerably lessened; while, on the other hand, there can be little doubt, although the reader may, perhaps, smile at the bare suggestion of such an idea, that an amphibious being, who should possess our faculties, would still more easily arrive at sound theoretical opinions in geology, since he might behold, on the one hand, the decomposition of rocks in the atmosphere or the transportation of matter by running water; and, on the other, examine the deposition of sediment in the sea and the imbedding of animal and vegetable remains in new strata. He might ascertain, by direct observation, the action of a mountain torrent as well as of a marine current; might compare the products of volcanos poured out upon the land with those ejected beneath the waters; and might mark, on the one hand, the growth of the forest, and, on the other, that of the coral reef. Yet, even with these advantages, he would be liable to fall into the greatest errors, when endeavouring to reason on rocks of subterranean origin. He would seek in vain, within the sphere of his observation, for any direct analogy to the process of their formation, and would therefore be in danger of attributing them, wherever they are upraised to view, to some "primeval state of nature."

For more than two centuries the shelly strata of the Sub-Apennine hills afforded matter of speculation to the early geologists of Italy, and few of them had any suspicion that similar deposits were then forming in the neighbouring sea. Some imagined that the strata, so rich in organic remains, instead of being due to secondary agents, had been so created in the beginning by the fiat of the Almighty. Others ascribed the imbedded fossil bodies to some plastic power which resided in the earth in the early ages of the world. In what manner were these dogmas at length exploded? The fossil relics were carefully compared with their living analogues, and all doubts as to their organic origin were eventually dispelled. So, also, in regard to the containing beds of mud, sand, and limestone: those parts of the bottom of the sea were examined where shells are now becoming annually entombed in new deposits. Donati explored the bed of the Adriatic and found the closest resemblance between the strata there forming and those which constituted hills above a thousand feet high in various parts of the Italian peninsula. He ascertained by dredging that living testacea were there grouped together in precisely the same manner

as were their fossil analogues in the inland strata; and while some of the recent shells of the Adriatic were becoming incrusted with calcareous rock, he discovered that others had been newly buried in sand and clay, precisely as fossil shells occur in the Sub-Apennine hills.

The establishment, from time to time, of numerous points of identification drew at length from geologists a reluctant admission that there was more correspondence between the condition of the globe at former eras and now, and more uniformity in the laws which have regulated the changes of its surface, than they at first imagined. If, in this state of the science, they still despaired of reconciling every class of geological phenomena to the operations of ordinary causes, even by straining analogy to the utmost limits of credibility, we might have expected, at least, that the balance of probability would now have been presumed to incline toward the close analogy of the ancient and modern causes. But, after repeated experience of the failure of attempts to speculate on geological monuments, as belonging to a distinct order of things, new sects continued to persevere in the principles adopted by their predecessors. They still began, as each new problem presented itself, whether relating to the animate or inanimate world, to assume an original and dissimilar order of nature; and when at length they approximated, or entirely came round to an opposite opinion, it was always with the feeling that they were conceding what they had been justified *a priori* in deeming improbable. In a word, the same men who, as natural philosophers, would have been most incredulous respecting any deviations from the known course of nature, if reported to have happened *in their own time,* were equally disposed, as geologists, to expect the proofs of such deviation at every period of the past.

III. Doctrine of the Discordance of the Ancient and Modern Causes of Change Controverted

Climate of the northern hemisphere formerly different.—Proofs of former revolutions in climate, as deduced from fossil remains, have afforded one of the most popular objections to the theory which endeavours to explain all geological changes by reference to those now in progress on the earth. The probable causes, therefore, of fluctuations in climate may first be treated of.

That the climate of the northern hemisphere has undergone an important change, and that its mean annual temperature must once have more nearly resembled that now experienced within the tropics, was the opinion of some of the first naturalists who investigated the contents of the ancient strata. Their conjecture became more probable when the shells and corals of the older tertiary and many secondary rocks were

carefully examined; for the organic remains of these formations were found to be intimately connected by generic affinity with species now living in warmer latitudes. At a later period, many reptiles, such as turtles, tortoises, and large saurian animals, were discovered in European formations in great abundance; and they supplied new and powerful arguments, from analogy, in support of the doctrine that the heat of the climate had been great when our secondary strata were deposited. Lastly, when the botanist turned his attention to the specific determination of fossil plants, the evidence acquired still further confirmation; for the flora of a country is peculiarly influenced by temperature: and the ancient vegetation of the earth might have been expected more readily than the forms of animals to have afforded conflicting proofs, had the popular theory been without foundation. When the examination of fossil remains was extended to rocks in the most northern parts of Europe and North America, and even to the Arctic regions, indications of the same revolution in climate were discovered.

Proofs from fossil shells in tertiary strata.—In Sicily, Calabria, and in the neighbourhood of Naples, the fossil testacea of the most modern tertiary formations belong almost entirely to species now inhabiting the Mediterranean; but as we proceed northwards in the Italian peninsula we find in the strata called Sub-Apennine an assemblage of fossil shells departing somewhat more widely from the type of the neighbouring seas. The proportion of species identifiable with those now living in the Mediterranean is still considerable; but it no longer predominates, as in the South of Italy and part of Sicily, over the unknown species. Although occurring in localities which are removed several degrees farther from the equator (as at Siena, Parmi, Asti, &c.), the shells yield clear indications of a warmer climate. This evidence is of great weight, and is not neutralized by any facts of a conflicting character; such, for instance, as the association, in the same group, of individuals referable to species now confined to arctic regions.

On comparing the fossils of the tertiary deposits of Paris and London with those of Bordeaux, and these again with the more modern strata of Sicily, we should at first expect that they would each indicate a higher temperature in proportion as they are situated farther to the south. But the contrary is true; of the shells belonging to these several groups, whether freshwater or marine, some are of extinct, others of living species. Those found in the older, or Eocene, deposits of Paris and London, although six or seven degrees to the north of the Miocene strata at Bordeaux, afford evidence of a warmer climate; while those of Bordeaux imply that the sea in which they lived was of a higher temperature than that of Sicily, where the shelly strata were formed six or seven degrees nearer to the equator. In these cases the greater antiquity of the several formations (the Parisian being the oldest and the Sicilian the newest) has more than counterbalanced the influence which latitude would otherwise exert, and this phenomenon clearly points to a gradual and successive refrigeration of climate.

Siberian mammoths.—It will naturally be asked whether some recent geological discoveries bringing evidence to light of a colder, or, as it has been termed, "glacial epoch," towards the close of the tertiary periods throughout the northern hemisphere, does not conflict with the theory above alluded to, of a warmer temperature having prevailed in the eras of the Eocene, Miocene, and Pliocene formations. In answer to this enquiry, it may certainly be affirmed that an oscillation of climate has occurred in times immediately antecedent to the peopling of the earth by man; but proof of the intercalation of a less genial climate at an era when nearly all the marine and terrestrial testacea had already become specifically the same as those now living by no means rebuts the conclusion previously drawn, in favour of a warmer condition of the globe, during the ages which elapsed while the tertiary strata were deposited. In some of the most superficial patches of sand, gravel, and loam, scattered very generally over Europe, and containing recent shells, the remains of extinct species of land quadrupeds have been found, especially in places where the alluvial matter appears to have been washed into small lakes or into depressions in the plains bordering ancient rivers. Among the extinct mammalia thus entombed, we find species of the elephant, rhinoceros, hippopotamus, bear, hyæna, lion, tiger, monkey (macacus), and many others; consisting partly of genera now confined to warmer regions.

It is certainly probable that when some of these quadrupeds abounded in Europe, the climate was milder than that now experienced. The hippopotamus, for example, is now only met with where the temperature of the water is warm and nearly uniform throughout the year, and where the rivers are never frozen over. Yet when the great fossil species (*Hippotamus major* Cuv.) inhabited England, the testacea of our country were nearly the same as those now existing, and the climate cannot be supposed to have been very hot.

The mammoth also appears to have existed in England when the temperature of our latitudes could not have been very different from that which now prevails; for remains of this animal have been found at North Cliff, in the county of York, in a lacustrine formation, in which all the land and freshwater shells, thirteen in number, can be identified with species and varieties now existing in that county. Bones of the bison also, an animal now inhabiting a cold or temperate climate, have been found in the same place. That these quadrupeds, and the indigenous species of testacea associated with them, were all contemporary inhabitants of Yorkshire has been established by unequivocal proof.

Recent investigations have placed beyond all doubt the important fact that a species of tiger, identical with that of Bengal, is common in the neighbourhood of Lake Aral, near Sussac, in the forty-fifth degree of north latitude; and from time to time this animal is now seen in Siberia in a latitude as far north as the parallel of Berlin and Hamburg.

Now, if the Indian tiger can range in our own times to the southern borders of Siberia or skirt the snows of the Himalaya, and if the puma can reach the fifty-third degree of latitude in South America, we may

easily understand how large species of the same genera may once have inhabited our temperate climate. The mammoth (*E. primigenius*), already alluded to, as occurring fossil in England, was decidedly different from the two existing species of elephants, one of which is limited to Asia, south of the thirty-first degree of north latitude, the other to Africa, where it extends as far south as the Cape of Good Hope.

Pallas and other writers describe the bones of the mammoth as abounding throughout all the Lowland of Siberia, stretching in a direction west and east, from the borders of Europe to the extreme point nearest America, and south and north, from the base of the mountains of Central Asia to the shores of the Arctic Sea. Within this space, scarcely inferior in area to the whole of Europe, fossil ivory has been collected almost everywhere, on the banks of the Irtish, Obi, Yenesei, and other rivers. But it is not on the Obi nor the Yenesei, but on the Lena, farther to the east, where, in the same parallels of latitude, the cold is far more intense, that fossil remains have been found in the most wonderful state of preservation. In 1772, Pallas obtained from Wiljuiskoi, in latitude 64°, from the banks of the Wiljui, a tributary of the Lena, the carcass of a rhinoceros (*R. tichorhinus*), taken from the sand in which it must have remained congealed for ages, the soil of that region being always frozen to within a slight depth of the surface. This carcass was compared to a natural mummy, and emitted an odour like putrid flesh, part of the skin being still covered with black and grey hairs.

After more than thirty years, the entire carcass of a mammoth (or extinct species of elephant) was obtained in 1803, by Mr. Adams, much farther to the north. It fell from a mass of ice, in which it had been encased, on the banks of the Lena, in latitude 70°; and so perfectly had the soft parts of the carcass been preserved that the flesh, as it lay, was devoured by wolves and bears. This skeleton is still in the museum of St. Petersburg, the head retaining its integument and many of the ligaments entire. The skin of the animal was covered, first, with black bristles, thicker than horsehair, from twelve to sixteen inches in length; secondly, with hair of a reddish-brown colour, about four inches long; and thirdly, with wool of the same colour as the hair, about an inch in length. Of the fur, upwards of thirty pounds' weight were gathered from the wet sand-bank. The individual was nine feet high and sixteen feet long, without reckoning the large curved tusks: a size rarely surpassed by the largest living male elephants.

It is evident, then, that the mammoth, instead of being naked, like the living Indian and African elephants, was enveloped in a thick shaggy covering of fur, probably as impenetrable to rain and cold as that of the musk ox. The species may have been fitted by nature to withstand the vicissitudes of a northern climate; and it is certain that, from the moment when the carcasses, both of the rhinoceros and elephant, above described, were buried in Siberia, in latitudes 64° and 70° north, the soil must have remained frozen and the atmosphere nearly as cold as at this day.

On considering all the facts above enumerated, it seems reasonable

to imagine that a large region in Central Asia, including, perhaps, the southern half of Siberia, enjoyed, at no very remote period in the earth's history, a temperate climate, sufficiently mild to afford food for numerous herds of elephants and rhinoceroses, *of species distinct from those now living*. But the age of this fauna was comparatively modern in the earth's history. It appears that when the oldest or Eocene tertiary deposits were formed, a warm temperature pervaded the European seas and lands. Shells of the genus Nautilus and other forms characteristic of tropical latitudes; fossil reptiles, such as the crocodile, turtle, and tortoise; plants, such as palms, some of them allied to the cocoanut, the screw pine, the custard apple, and the acacia, all lead to this conclusion. This flora and fauna were followed by those of the Miocene formation, in which indications of a southern, but less tropical, climate are detected. Finally, the Pliocene deposits, which come next in succession, exhibit in their organic remains a much nearer approach to the state of things now prevailing in corresponding latitudes. It was towards the close of this period that the seas of the northern hemisphere became more and more filled with floating icebergs often charged with erratic blocks, so that the waters and the atmosphere were chilled by the melting ice, and an arctic fauna enabled, for a time, to invade the temperate latitudes both of North America and Europe. The extinction of a considerable number of land quadrupeds and aquatic mollusca was gradually brought about by the increasing severity of the cold; but many species survived this revolution in climate, either by their capacity of living under a variety of conditions, or by migrating for a time to more southern lands and seas. At length, by modifications in the physical geography of the northern regions, and the cessation of floating ice on the eastern side of the Atlantic, the cold was moderated, and a milder climate ensued, such as we now enjoy in Europe.

Proofs from fossils in secondary and still older strata.—A great interval of time appears to have elapsed between the formation of the secondary strata, which constitute the principal portion of the elevated land in Europe, and the origin of the Eocene deposits. If we examine the rocks from the chalk to the new red sandstone inclusive, we find many distinct assemblages of fossils entombed in them, all of unknown species, and many of them referable to genera and families now most abundant between the tropics. Among the most remarkable are reptiles of gigantic size; some of them herbivorous, others carnivorous, and far exceeding in size any now known even in the torrid zone. The genera are for the most part extinct, but some of them, as the crocodile and monitor, have still representatives in the warmer parts of the earth. Coral reefs also were evidently numerous in the seas of the same periods, composed of species often belonging to genera now characteristic of a tropical climate. The number of large chambered shells also, including the nautilus, leads us to infer an elevated temperature; and the associated fossil plants, although imperfectly known, tend to the same conclusion, the Cycadeæ constituting the most numerous family.

But it is from the more ancient coal deposits that the most extraordi-

nary evidence has been supplied in proof of the former existence of a very different climate, a climate which seems to have been moist, warm, and extremely uniform, in those very latitudes which are now the colder, and in regard to temperature the most variable regions of the globe. We learn from the researches of Adolphe Brongniart, Goeppert, and other botanists that in the flora of the Carboniferous era there was a great predominance of ferns, some of which were arborescent; as, for example, Caulopteris, Protopteris, and Psarronius; nor can this be accounted for, as some have supposed, by the greater power which ferns possess of resisting maceration in water. This prevalence of ferns indicates a moist, equable, and temperate climate, and the absence of any severe cold; for such are the conditions which, at the present day, are found to be most favourable to that tribe of plants. It is only in the islands of the tropical oceans, and of the southern temperate zone, such as Norfolk Island, Otaheite, the Sandwich Islands, Tristan d'Acunha, and New Zealand, that we find any near approach to that remarkable preponderance of ferns which is characteristic of the carboniferous flora. It has been observed that tree ferns and other forms of vegetation which flourished most luxuriantly within the tropics extend to a much greater distance from the equator in the southern hemisphere than in the northern, being found even as far as 46° south latitude in New Zealand. There is little doubt that this is owing to the more uniform and moist climate occasioned by the greater proportional area of sea. Next to ferns and pines, the most abundant vegetable forms in the coal formation are the Calamites, Lepidodendra, Sigillariæ, and Stigmariæ. These were formerly considered to be so closely allied to tropical genera, and to be so much greater in size than the corresponding tribes now inhabiting equatorial latitudes, that they were thought to imply an extremely hot as well as humid and equable climate. But recent discoveries respecting the structure and relations of these fossil plants have shown that they deviated so widely from all existing types in the vegetable world that we have more reason to infer from this evidence a widely different climate in the Carboniferous era, as compared to that now prevailing, than a temperature extremely elevated. Palms, if not entirely wanting when the strata of the carboniferous group were deposited, appear to have been exceedingly rare. The Coniferæ, on the other hand, so abundantly met with in the coal, resemble Araucariæ in structure, a family of the fir tribe characteristic at present of the milder regions of the southern hemisphere, such as Chili, Brazil, New Holland, and Norfolk Island.

"In regard to the geographical extent of the ancient vegetation, it was not confined," says M. Brongniart, "to a small space, as to Europe, for example; for the same forms are met with again at great distances. Thus the coal plants of North America are, for the most part, identical with those of Europe, and all belong to the same genera. Some specimens, also, from Greenland, are referable to ferns, analogous to those of our European coal mines."

To return, therefore, from this digression—the flora of the coal appears to indicate a uniform and mild temperature in the air, while the

fossils of the contemporaneous mountain limestone, comprising abundance of lamelliferous corals, large chambered cephalopods, and crinoidea, naturally lead us to infer a considerable warmth in the waters of the northern sea of the Carboniferous period. So also in regard to strata older than the coal, they contain in high northern latitudes mountain masses of corals which must have lived and grown on the spot, and large chambered univalves, such as Orthocerata and Nautilus, all seeming to indicate, even in regions bordering on the arctic circle, the former prevalence of a temperature more elevated than that now prevailing.

The warmth and humidity of the air, and the uniformity of climate, both in the different seasons of the year and in different latitudes, appear to have been most remarkable when some of the oldest of the fossiliferous strata were formed. The approximation to a climate similar to that now enjoyed in these latitudes does not commence till the era of the formations termed tertiary; and while the different tertiary rocks were deposited in succession, from the Eocene to the Pliocene, the temperature seems to have been lowered, and to have continued to diminish even after the appearance upon the earth of a considerable number of the existing species, the cold reaching its maximum of intensity in European latitudes during the glacial epoch, or the epoch immediately antecedent to that in which all the species now contemporary with man were in being.

IV. Farther Examination of the Question as to the Assumed Discordance of the Ancient and Modern Causes of Change

Causes of vicissitudes in climate.—As the proofs enumerated in the last chapter indicate that the earth's surface has experienced great changes of climate since the deposition of the older sedimentary strata, we have next to inquire how such vicissitudes can be reconciled with the existing order of nature. At first it was imagined that the earth's axis had been for ages perpendicular to the plane of the ecliptic, so that there was a perpetual equinox, and uniformity of seasons throughout the year;—that the planet enjoyed this "paradisiacal" state until the era of the great flood; but in that catastrophe, whether by the shock of a comet or some other convulsion, it lost its equal poise, and hence the obliquity of its axis, and with that the varied seasons of the temperate zone and the long nights and days of the polar circles.

When the progress of astronomical science had exploded this theory, it was assumed that the earth at its creation was in a state of fluidity and red hot, and that ever since that era, it had been cooling down, contracting its dimensions, and acquiring a solid crust—an hypothesis hardly less

arbitrary, yet more calculated for lasting popularity; because, by referring the mind directly to the beginning of things, it requires no support from observation, nor from any ulterior hypothesis. But if, instead of forming vague conjectures as to what might have been the state of the planet at the era of its creation, we fix our thoughts on the connexion at present existing between climate and the distribution of land and sea, and then consider what influence former fluctuations in the physical geography of the earth must have had on superficial temperature, we may perhaps approximate to a true theory. If doubts and obscurities still remain, they should be ascribed to our limited acquaintance with the laws of Nature, not to revolutions in her economy;—they should stimulate us to farther research, not tempt us to indulge our fancies respecting the imaginary changes of internal temperature in an embryo world.

Diffusion of heat over the globe.—In considering the laws which regulate the diffusion of heat over the globe, we must be careful, as Humboldt well remarks, not to regard the climate of Europe as a type of the temperature which all countries placed under the same latitude enjoy. For the same reason, we may warn the geologist to be on his guard, and not hastily to assume that the temperature of the earth in the present era is a type of that which most usually obtains, since he contemplates far mightier alterations in the position of land and sea, at different epochs, than those which now cause the climate of Europe to differ from that of other countries in the same parallels.

On comparing the two continents of Europe and America, it is found that places in the same latitudes have sometimes a mean difference of temperature amounting to 11°, or even in a few cases to 17° Fahrenheit; and some places on the two continents, which have the same mean temperature, differ from 7° to 17° in latitude. Thus, Cumberland House, in North America, having the same latitude (54° north) as the city of York in England, stands on the isothermal line of 32°, which in Europe rises to the North Cape, in latitude 71°, but its summer heat exceeds that of Brussels or Paris. The principal cause of greater intensity of cold in corresponding latitudes of North America, as contrasted with Europe, is the connexion of America with the polar circle, by a large tract of land, some of which is from three to five thousand feet in height; and, on the other hand, the separation of Europe from the arctic circle by an ocean. The ocean has a tendency to preserve everywhere a mean temperature, which it communicates to the contiguous land, so that it tempers the climate, moderating alike an excess of heat or cold. The elevated land, on the other hand, rising to the colder regions of the atmosphere, becomes a great reservoir of ice and snow, arrests, condenses, and congeals vapour, and communicates its cold to the adjoining country. For this reason, Greenland, forming part of a continent which stretches northward to the 82d degree of latitude, experiences under the 60th parallel a more rigorous climate than Lapland under the 72d parallel.

But if land be situated between the 40th parallel and the equator, it produces, unless it be of extreme height, exactly the opposite effect; for

it then warms the tracts of land or sea that intervene between it and the polar circle. For the surface, being in this case exposed to the vertical, or nearly vertical, rays of the sun, absorbs a large quantity of heat, which it diffuses by radiation into the atmosphere. For this reason, the western parts of the old continent derive warmth from Africa; "which, like an immense furnace, distributes its heat to Arabia, to Turkey in Asia, and to Europe." On the contrary, the northeastern extremity of Asia experiences in the same latitude extreme cold; for it has land on the north between the 60th and 70th parallel, while to the south it is separated from the equator by the Pacific Ocean.

Influence of currents on temperature.—Among other influential causes, both of remarkable diversity in the mean annual heat, and of unequal division of heat in the different seasons, are the direction of currents and the accumulation and drifting of ice in high latitudes. The temperature of the Lagullas current is 10° or 12° Fahrenheit above that of the sea at the Cape of Good Hope; for it derives the greater part of its waters from the Mozambique Channel, and southeast coast of Africa, and from regions in the Indian Ocean much nearer the line, and much hotter than the Cape. An opposite effect is produced by the "equatorial" current, which crosses the Atlantic from Africa to Brazil, having a breadth varying from 160 to 450 nautical miles. Its waters are cooler by 3° or 4° Fahrenheit than those of the ocean under the line, so that it moderates the heat of the tropics.

But the effects of the Gulf Stream on the climate of the North Atlantic Ocean are far more remarkable. This most powerful of known currents has its source in the Gulf or Sea of Mexico, which, like the Mediterranean and other close seas in temperate or low latitudes, is warmer than the open ocean in the same parallels. The temperature of the Mexican sea in summer is 86° Fahrenheit, or at least 7° above that of the Atlantic in the same latitude. From this great reservoir or caldron of warm water a constant current pours forth through the Straits of Bahama at the rate of 3 or 4 miles an hour; it crosses the ocean in a northeasterly direction, skirting the great bank of Newfoundland, where it still retains a temperature of 8° above that of the surrounding sea. It reaches the Azores in about 78 days, after flowing nearly 3000 geographical miles, and from thence it sometimes extends its course a thousand miles farther, so as to reach the Bay of Biscay, still retaining an excess of 5° above the mean temperature of that sea. As it has been known to arrive there in the months of November and January, it may tend greatly to moderate the cold of winter in countries on the west of Europe.

Difference of climate of the northern and southern hemispheres.—When we compare the climate of the northern and southern hemispheres, we obtain still more instruction in regard to the influence of the distribution of land and sea on climate. The dry land in the southern hemisphere is to that of the northern in the ratio only of one to three, excluding from our consideration that part which lies between the pole and the 78° of south latitude, which has hitherto proved inaccessible. And whereas in the northern hemisphere, between the pole and the thirtieth parallel of

north latitude, the land and sea occupy nearly equal areas, the ocean in the southern hemisphere covers no less than fifteen parts in sixteen of the entire space included between the antarctic circle and the thirtieth parallel of south latitude.

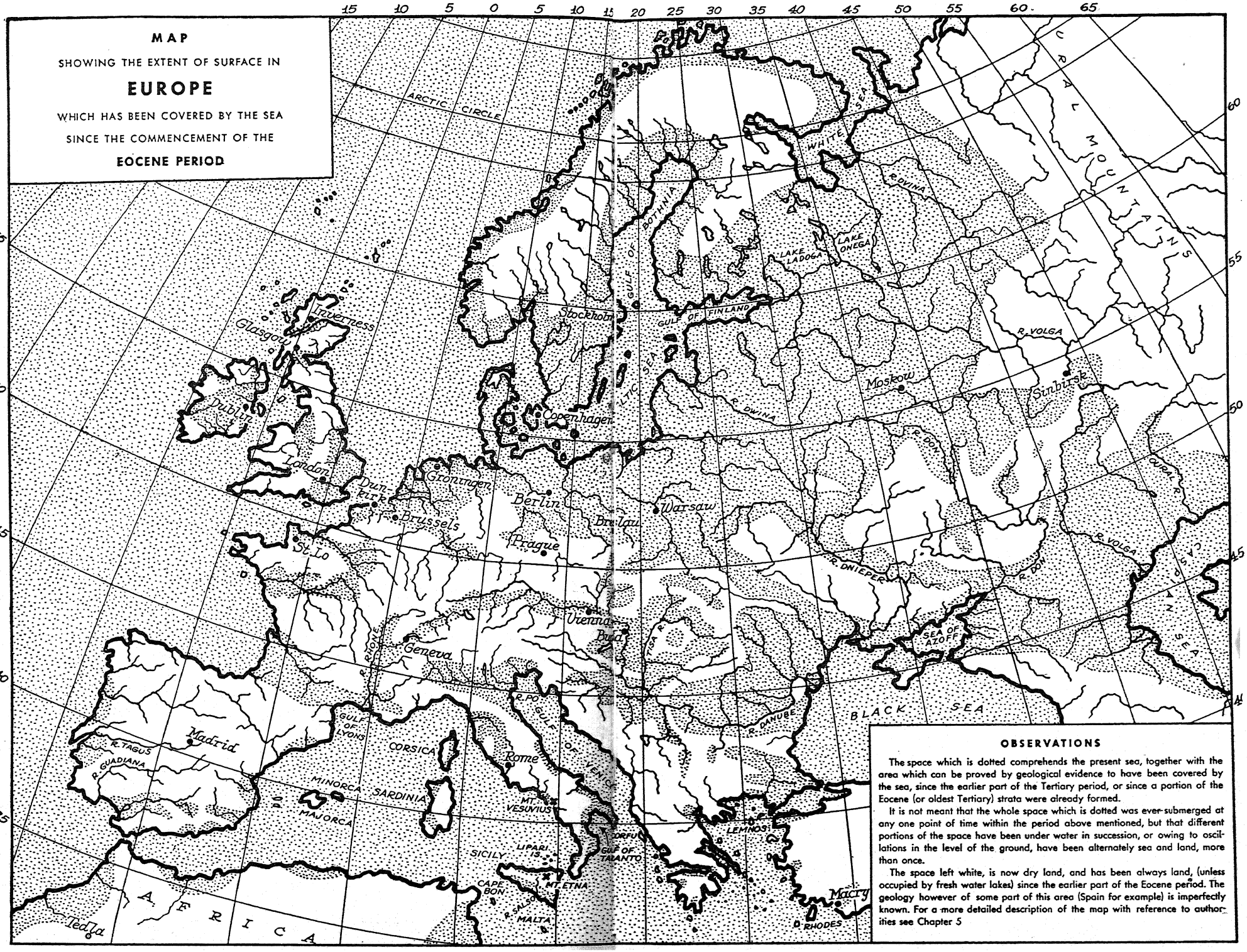
This great extent of sea gives a particular character to climates south of the equator, the winters being mild and the summers cool. Thus, in Van Diemen's Land, corresponding nearly in latitude to Rome, the winters are more mild than at Naples, and the summers not warmer than those at Paris, which is 7° farther from the equator.

It has long been supposed that the general temperature of the southern hemisphere was considerably lower than that of the northern, and that the difference amounted to at least 10° Fahrenheit. Baron Humboldt, after collecting and comparing a great number of observations, came to the conclusion that even a much larger difference existed, but that none was to be observed within the tropics, and only a small difference as far as the thirty-fifth and fortieth parallel.

The description given by ancient as well as modern navigators of the sea and land in high southern latitudes clearly attests the greater severity of the climate as compared to arctic regions. In Sandwich Land, in latitude 59° south, or in nearly the same parallel as the north of Scotland, Captain Cook found the whole country, from the summits of the mountains down to the very brink of the sea cliffs, "covered many fathoms thick with everlasting snow," and this on the 1st of February, the hottest time of the year. The permanence of snow in the southern hemisphere is in this instance partly due to the floating ice, which chills the atmosphere and condenses the vapour, so that in summer the sun cannot pierce through the foggy air. But besides the abundance of ice which covers the sea to the south of Georgia and Sandwich Land, we may also, as Humboldt suggests, ascribe the cold of those countries in part to the absence of land between them and the tropics.

If Africa and New Holland extended farther to the south, a diminution of ice would take place in consequence of the radiation of heat from these continents during summer, which would warm the contiguous sea and rarefy the air. The heated aerial currents would then ascend and flow more rapidly towards the south pole, and moderate the winter. In confirmation of these views, it is stated that the ice, which extends as far as the 68° and 71° of south latitude, advances more towards the equator whenever it meets an open sea; that is, where the extremities of the present continents are not opposite to it; and this circumstance seems explicable only on the principle above alluded to, of the radiation of heat from the lands so situated.

The cold of the antarctic regions was conjectured by Cook to be due to the existence of a large tract of land between the seventieth degree of south latitude and the pole. The justness of these and other speculations of that great navigator have since been singularly confirmed by the investigation made by Sir James Ross in 1841. He found Victoria Land, extending from 71° to 79° south latitude, skirted by a great barrier of ice, the


MAP
SHOWING THE EXTENT OF SURFACE IN
EUROPE
WHICH HAS BEEN COVERED BY THE SEA
SINCE THE COMMENCEMENT OF THE
EOCENE PERIOD
ARCTIC CIRCLE
WHITE SEA
R. DVINA
LAKE LADOGA
LAKE ONEGA
GULF OF BOTHNIA
GULF OF FINLAND
BALTIC SEA
URAL MOUNTAINS
R. VOLGA
Stockholm
Copenhagen
Moskow
Sinbirsk
Glasgow
Inverness
Dublin
London
Dunkirk
Groningen
Brussels
Berlin
Breslau
Warsaw
Prague
St. Lo
Vienna
Buda
Geneva
R. RHONE
R. DNIEPER
R. DON
CASPIAN SEA
SEA OF AZOFF
BLACK SEA
R. DANUBE
R. PO
GULF OF VENICE
GULF OF LYONS
CORSICA
Madrid
R. TAGUS
R. GUADIANA
MINORCA
SARDINIA
MAJORCA
Rome
MT. VESUVIUS
SICILY
LIPARI IS.
MT. ETNA
CAPE BON
MALTA
GULF OF TARANTO
CORFU
LEMNOS
Macry
RHODES
Tedla
AFRICA
OBSERVATIONS
The space which is dotted comprehends the present sea, together with the area which can be proved by geological evidence to have been covered by the sea, since the earlier part of the Tertiary period, or since a portion of the Eocene (or oldest Tertiary) strata were already formed.
It is not meant that the whole space which is dotted was ever submerged at any one point of time within the period above mentioned, but that different portions of the space have been under water in succession, or owing to oscillations in the level of the ground, have been alternately sea and land, more than once.
The space left white, is now dry land, and has been always land, (unless occupied by fresh water lakes) since the earlier part of the Eocene period. The geology however of some part of this area (Spain for example) is imperfectly known. For a more detailed description of the map with reference to authorities see Chapter 5

height of the land ranging from 4000 to 14,000 feet, the whole entirely covered with snow, except a narrow ring of black earth surrounding the huge crater of the active volcano of Mount Erebus, rising 12,400 feet above the level of the sea.

Changes in the position of land and sea may give rise to vicissitudes in climate.—Having offered these brief remarks on the diffusion of heat over the globe in the present state of the surface, I shall now proceed to speculate on the vicissitudes of climate, which must attend those endless variations in the geographical features of our planet which are contemplated in geology. That our speculations may be confined within the strict limits of analogy, I shall assume, 1st, That the proportion of dry land to sea continues always the same. 2dly, That the volume of the land rising above the level of the sea is a constant quantity; and not only that its mean, but that its extreme height, is liable only to trifling variations. 3dly, That both the mean and extreme depth of the sea are invariable; and 4thly, It may be consistent with due caution to assume that the grouping together of the land in continents is a necessary part of the economy of nature; for it is possible that the laws which govern the subterranean forces, and which act simultaneously along certain lines, cannot but produce, at every epoch, continuous mountain chains; so that the subdivision of the whole land into innumerable islands may be precluded.

Before considering the effect which a material change in the distribution of land and sea must occasion, it may be well to remark how greatly organic life may be affected by those minor variations, which need not in the least degree alter the general temperature. Thus, for example, if we suppose, by a series of convulsions, a certain part of Greenland to become sea, and, in compensation, a tract of land to rise and connect Spitzbergen with Lapland—an accession not greater in amount than one which the geologist can prove to have occurred in certain districts bordering the Mediterranean, within a comparatively modern period—this altered form of the land might cause an interchange between the climate of certain parts of North America and of Europe, which lie in corresponding latitudes. Many European species of plants and animals would probably perish in consequence, because the mean temperature would be greatly lowered; and others would fail in America, because it would there be raised. On the other hand, in places where the mean annual heat remained unaltered, some species which flourish in Europe, where the seasons are more uniform, would be unable to resist the greater heat of the North American summer, or the intenser cold of the winter; while others, now fitted by their habits for the great contrast of the American seasons, would not be fitted for the *insular* climate of Europe.

If we now proceed to consider the circumstances required for a *general* change of temperature, it will appear, from the facts and principles already laid down, that whenever a greater extent of high land is collected in the polar regions, the cold will augment; and the same result will be produced when there is more sea between or near the tropics; while, on the contrary, so often as the above conditions are reversed, the heat will

be greater. (See Figs. 3 and 4.) If this be admitted, it will follow that unless the superficial inequalities of the earth be fixed and permanent, there must be never-ending fluctuations in the mean temperature of every zone; and that the climate of one era can no more be a type of every other than is one of our four seasons of all the rest.

Position of land and sea which might produce the extreme of cold of which the earth's surface is susceptible.—To simplify our view of the various changes in climate, which different combinations of geographical circumstances may produce, we shall first consider the conditions necessary for bringing about the extreme of cold, or what would have been termed in the language of the old writers the winter of the "great year," or geological cycle, and afterwards, the conditions requisite to produce the maximum of heat, or the summer of the same year.

To begin with the northern hemisphere. Let us suppose those hills of the Italian peninsula and of Sicily, which are of comparatively modern origin, and contain many fossil shells identical with living species, to subside again into the sea, from which they have been raised, and that an extent of land of equal area and height (varying from one to three thousand feet) should rise up in the Arctic Ocean between Siberia and the north pole. The alteration now supposed in the physical geography of the northern regions would cause additional snow and ice to accumulate where now there is usually an open sea; and the temperature of the greater part of Europe would be somewhat lowered, so as to resemble more nearly that of corresponding latitudes of North America: or, in other words, it might be necessary to travel about 10° farther south in order to meet with the same climate which we now enjoy. No compensation would be derived from the disappearance of land in the Mediterranean countries; but the contrary, since the mean heat of the soil in those latitudes probably exceeds that which would belong to the sea, by which we imagine it to be replaced.

But let the configuration of the surface be still farther varied, and let some large district within or near the tropics, such as Brazil, with its plains and hills of moderate height, be converted into sea, while lands of equal elevation and extent rise up in the arctic circle. From this change there would, in the first place, result a sensible diminution of temperature near the tropic, for the Brazilian soil would no longer be heated by the sun; so that the atmosphere would be less warm, as also the neighbouring Atlantic. On the other hand, the whole of Europe, Northern Asia, and North America would be chilled by the enormous quantity of ice and snow thus generated on the new arctic continent. If, as we have already seen, there are now some points in the southern hemisphere where snow is perpetual down to the level of the sea, in latitudes as low as central England, such might assuredly be the case throughout a great part of Europe, under the change of circumstances above supposed: and if at present the extreme range of drifted icebergs is the Azores, they might easily reach the equator after the assumed alteration. But to pursue the subject still further, let the Himalaya Mountains, with the whole of

Hindostan, sink down, and their place be occupied by the Indian Ocean, while an equal extent of territory and mountains, of the same vast height, rise up between North Greenland and the Orkney Islands. It seems difficult to exaggerate the amount to which the climate of the northern hemisphere would then be cooled.

But the refrigeration brought about at the same time in the southern hemisphere would be nearly equal, and the difference of temperature between the arctic and equatorial latitudes would not be much greater than at present; for no important disturbance can occur in the climate of a particular region without its immediately affecting all other latitudes, however remote. The heat and cold which surround the globe are in a state of constant and universal flux and reflux. The heated and rarefied air is always rising and flowing from the equator towards the poles in the higher regions of the atmosphere; while in the lower, the colder air is flowing back to restore the equilibrium.

That a corresponding interchange takes place in the seas is demonstrated, according to Humboldt, by the cold which is found to exist at great depths within the tropics; and, among other proofs, may be mentioned the mass of warmer water which the Gulf Stream is constantly bearing northwards, while a cooler current flows *from* the north along the coast of Greenland and Labrador and helps to restore the equilibrium.

To return to the state of the earth after the changes above supposed, we must not omit to dwell on the important effects to which a wide expanse of perpetual snow would give rise. It is probable that nearly the whole sea, from the poles to the parallels of 45°, would be frozen over; for it is well known that the immediate proximity of land is not essential to the formation and increase of field ice, provided there be in some part of the same zone a sufficient quantity of glaciers generated on or near the land, to cool down the sea. Captain Scoresby, in his account of the arctic regions, observes that when the sun's rays "fall upon the snow-clad surface of the ice or land, they are in a great measure reflected, without producing any material elevation of temperature; but when they impinge on the black exterior of a ship, the pitch on one side occasionally becomes fluid while ice is rapidly generated at the other."

Now field ice is almost always covered with snow; and thus not only land as extensive as our existing continents, but immense tracts of sea in the frigid and temperate zones, might present a solid surface covered with snow, and reflecting the sun's rays for the greater part of the year. Within the tropics, moreover, where the ocean now predominates, the sky would no longer be serene and clear, as in the present era; but masses of floating ice would cause quick condensations of vapour, so that fogs and clouds would deprive the vertical rays of the sun of half their power. The whole planet, therefore, would receive annually a smaller proportion of the solar influence, and the external crust would part, by radiation, with some of the heat which had been accumulated in it during a different state of the surface. This heat would be dissipated in the spaces sur-

rounding our atmosphere, which, according to the calculations of M. Fourier, have a temperature much inferior to that of freezing water.

After the geographical revolution above assumed, the climate of equinoctial lands might be brought at last to resemble that of the present temperate zone, or perhaps be far more wintry. They who should then inhabit such small isles and coral reefs as are now seen in the Indian Ocean and South Pacific would wonder that zoophytes of large dimensions had once been so prolific in their seas; or if, perchance, they found the wood and fruit of the cocoanut tree or the palm silicified by the waters of some ancient mineral spring, or incrusted with calcareous matter, they would muse on the revolutions which had annihilated such genera and replaced them by the oak, the chestnut, and the pine. With equal admiration would they compare the skeletons of their small lizards with the bones of fossil alligators and crocodiles more than twenty feet in length, which, at a former epoch, had multiplied between the tropics: and when they saw a pine included in an iceberg, drifted from latitudes which we now call temperate, they would be astonished at the proof thus afforded that forests had once grown where nothing could be seen in their own times but a wilderness of snow.

But we have still to contemplate the additional refrigeration which might be effected by changes in the relative position of land and sea in the southern hemisphere. If the remaining continents were transferred from the equatorial and contiguous latitudes to the south polar regions, the intensity of cold produced might, perhaps, render the globe uninhabitable. We are too ignorant of the laws governing the direction of subterranean forces to determine whether such a crisis be within the limits of possibility. At the same time, it may be observed that no distribution of land can well be imagined more irregular, or, as it were, capricious, than that which now prevails; for at present, the globe may be divided into two equal parts, in such a manner that one hemisphere shall be almost entirely covered with water, while the other shall contain less water than land (see Figs. 1 and 2); and, what is still more extraordinary, on comparing the extratropical lands in the northern and southern hemispheres, the lands in the northern are found to be to those in the southern in the proportion of thirteen to one! To imagine all the lands, therefore, in high, and all the sea in low latitudes, as delineated in Figs. 3 and 4, would scarcely be a more anomalous state of the surface.

Position of land and sea which might give rise to the extreme of heat.—Let us now turn from the contemplation of the winter of the "great year," and consider the opposite train of circumstances which would bring on the spring and summer. To imagine all the lands to be collected together in equatorial latitudes, and a few promontories only to project beyond the thirtieth parallel, as represented in the annexed map (Fig. 3), would be undoubtedly to suppose an extreme result of geological change. But if we consider a mere approximation to such a state of things, it would be sufficient to cause a general elevation of temperature. Nor can it be regarded as a visionary idea that amidst the revolu-

FIG. 1

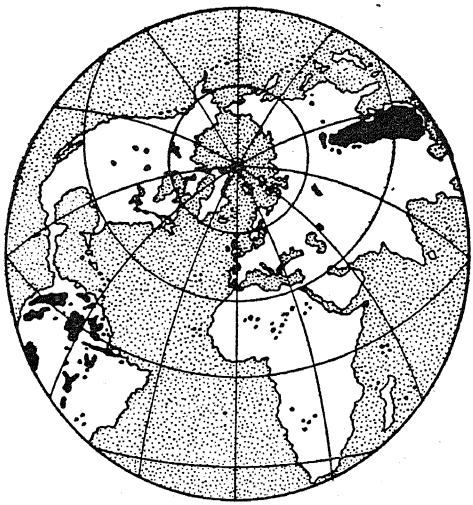
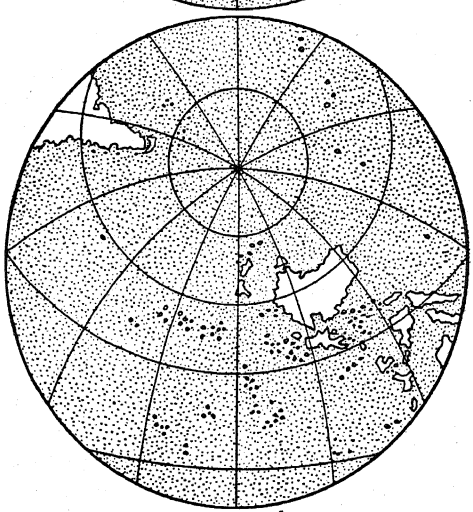


FIG. 2

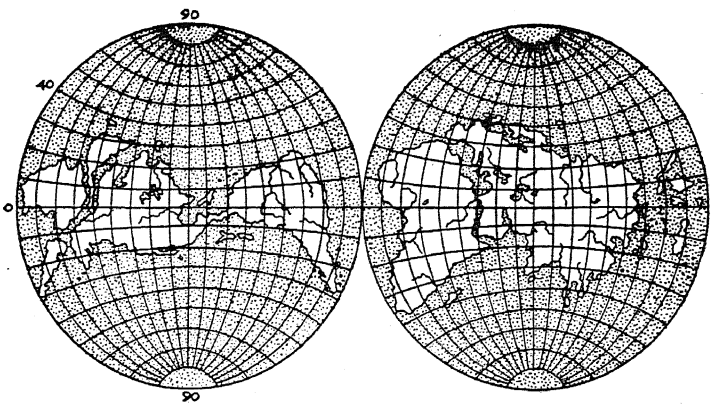


MAP SHOWING THE PRESENT UNEQUAL DISTRIBUTION OF LAND AND WATER ON THE SURFACE OF THE GLOBE

FIG. 1. Here London is taken as a centre and we behold the greatest quantity of land existing in one hemisphere.

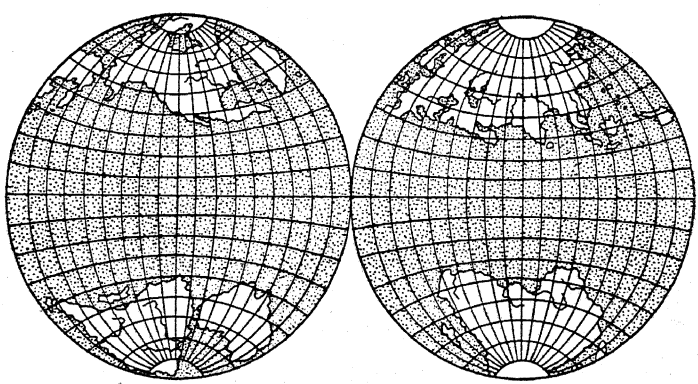
FIG. 2. Here the centre is the antipodal point to London and we see the greatest quantity of water existing in one hemisphere.

FIG. 3



EXTREME OF HEAT

FIG. 4



EXTREME OF COLD

MAPS SHOWING THE POSITION OF LAND AND SEA WHICH MIGHT PRODUCE THE EXTREMES OF HEAT AND COLD IN THE CLIMATES OF THE GLOBE

Observations: These maps are intended to show that continents and islands having the same shape and relative dimensions as those now existing might be placed so as to occupy either the equatorial or polar regions.

In FIG. 3 scarcely any of the land extends from the equator towards the poles beyond the 30th parallel of latitude, and in FIG. 4 a very small proportion of it extends from the poles towards the equator beyond the 40th parallel of latitude.

tions of the earth's surface the quantity of land should, at certain periods, have been simultaneously lessened in the vicinity of both the poles and increased within the tropics. We must recollect that even now it is necessary to ascend to the height of fifteen thousand feet in the Andes under the line, and in the Himalaya Mountains, which are without the tropic, to seventeen thousand feet, before we reach the limit of perpetual snow. On the northern slope, indeed, of the Himalaya range, where the heat radiated from a great continent moderates the cold, there are meadows and cultivated land at an elevation equal to the height of Mont Blanc. If then there were no arctic lands to chill the atmosphere and freeze the sea, and if the loftiest chains were near the line, it seems reasonable to imagine that the highest mountains might be clothed with a rich vegetation to their summits, and that nearly all signs of frost would disappear from the earth.

When the absorption of the solar rays was in no region impeded, even in winter, by a coat of snow, the mean heat of the earth's crust would augment to considerable depths, and springs, which we know to be in general an index of the mean temperature of the climate, would be warmer in all latitudes. The waters of lakes, therefore, and rivers, would be much hotter in winter, and would be never chilled in summer by melted snow and ice. A remarkable uniformity of climate would prevail amid the archipelagos of the temperate and polar oceans, where the tepid waters of equatorial currents would freely circulate. The general humidity of the atmosphere would far exceed that of the present period, for increased heat would promote evaporation in all parts of the globe. The winds would be first heated in their passage over the tropical plains, and would then gather moisture from the surface of the deep, till, charged with vapour, they arrived at extreme northern and southern regions, and there encountering a cooler atmosphere, discharged their burden in warm rain. If, during the long night of a polar winter, the snows should whiten the summits of some arctic islands, they would be dissolved as rapidly by the returning sun as are the snows of Etna by the blasts of the sirocco.

We learn from those who have studied the geographical distribution of plants that in very low latitudes, at present, the vegetation of small islands remote from continents has a peculiar character; the ferns and allied families, in particular, bearing a great proportion to the total number of other plants. Other circumstances being the same, the more remote the isles are from the continents, the greater does this proportion become. Thus, in the continent of India, and the tropical parts of New Holland, the proportion of ferns to the phænogamous plants is only as one to twenty-six; whereas, in the South Sea Islands, it is as one to four, or even as one to three.

We might expect, therefore, in the summer of the "great year," or cycle of climate, that there would be a predominance of tree ferns and plants allied to genera now called tropical, in the islands of the wide ocean, while many forms now confined to arctic and temperate regions, or only found near the equator on the summit of the loftiest mountains,

would almost disappear from the earth. Then might those genera of animals return, of which the memorials are preserved in the ancient rocks of our continents. The pterodactyle might flit again through the air, the huge iguanodon reappear in the woods, and the ichthyosaurs swarm once more in the sea. Coral reefs might be prolonged again beyond the arctic circle, where the whale and the narwal now abound; and droves of turtles might begin again to wander through regions now tenanted by the walrus and the seal.

But not to indulge too far in these speculations, I may observe, in conclusion, that however great, during the lapse of ages, may be the vicissitudes of temperature in every zone, it accords with this theory that the general climate should not experience any sensible change in the course of a few thousand years; because that period is insufficient to affect the leading features of the physical geography of the globe.

V. On Former Changes in Physical Geography and Climate

I HAVE STATED the arguments derived from organic remains for concluding that in the period when the carboniferous strata were deposited, the temperature of the ocean and the air was more uniform in the different seasons of the year, and in different latitudes, than at present, and that there was a remarkable absence of cold as well as great moisture in the atmosphere. It was also shown that the climate had been modified more than once since that epoch, and that it had been reduced, by successive changes, more and more nearly to that now prevailing in the same latitudes. Further, I endeavoured, in the last chapter, to prove that vicissitudes in climate of no less importance may be expected to recur in future if it be admitted that causes now active in nature have power, in the lapse of ages, to produce considerable variations in the relative position of land and sea. It remains to inquire whether the alterations, which the geologist can prove to have *actually taken place* at former periods, in the geographical features of the northern hemisphere, coincide in their nature, and in the time of their occurrence, with such revolutions in climate as might naturally have resulted, according to the meteorological principles already explained.

Period of the primary fossiliferous rocks.—The oldest system of strata which afford by their organic remains any evidence as to climate, or the former position of land and sea, are those formerly known as the *transition rocks,* or what have since been termed Lower Silurian or "primary fossiliferous" formations. These have been found in England, France, Germany, Sweden, Russia, and other parts of central and northern Europe, as also in the Great Lake District of Canada and the United States. The

multilocular or chambered univalves, including the Nautilus, and the corals, obtained from the limestones of these ancient groups, have been compared to forms now most largely developed in tropical seas. The corals, however, have been shown by M. Milne Edwards to differ generally from all living zoophytes; so that conclusions as to a warmer climate drawn from such remote analogies must be received with caution. Hitherto, few, if any, contemporaneous vegetable remains have been noticed; but such as are mentioned agree more nearly with the plants of the Carboniferous era than any other, and would therefore imply a warm and humid atmosphere entirely free from intense cold throughout the year.

This absence or great scarcity of plants as well as the freshwater shells and other indications of neighbouring land, coupled with the wide extent of marine strata of this age in Europe and North America, are facts which imply such a state of physical geography (so far at least as regards the northern hemisphere) as would, according to the principles before explained, give rise to such a moist and equable climate. (See Fig. 3.)

Carboniferous group.—This group comes next in the order of succession: and one of its principal members, the mountain limestone, was evidently a marine formation, as is shown by the shells and corals which it contains. That the ocean of that period was of considerable extent in our latitudes, we may infer from the continuity of these calcareous strata over large areas in Europe, Canada, and the United States. The same group has also been traced in North America, towards the borders of the arctic sea.

Since the time of the earlier writers, no strata have been more extensively investigated, both in Europe and North America, than those of the ancient carboniferous group, and the progress of science has led to a general belief that a large portion of the purest coal has been formed, not, as was once imagined, by vegetable matter floated from a distance, but by plants which grew on the spot, and somewhat in the manner of peat on the spaces now covered by the beds of coal. The former existence of land in some of these spaces has been proved, as already stated, by the occurrence of numerous upright fossil trees, with their roots terminating downwards in seams of coal; and still more generally by the roots of trees (stigmariæ) remaining in their natural position in the clays which underlie almost every layer of coal.

As some nearly continuous beds of such coal have of late years been traced in North America, over areas 100 or 200 miles and upwards in diameter, it may be asked whether the large tracts of ancient land implied by this fact are not inconsistent with the hypothesis of the general prevalence of islands at the period under consideration. In reply, I may observe that the coal fields must originally have been low alluvial grounds, resembling in situation the cypress swamps of the Mississippi, or the sunderbunds of the Ganges, being liable like them to be inundated at certain periods by a river or by the sea, if the land should be depressed a few feet. All the phenomena, organic and inorganic, imply conditions nowhere to be met with except in the deltas of large rivers. We have to account

for an abundant supply of fluviatile sediment, carried for ages towards one and the same region, and capable of forming strata of mud and sand thousands of feet, or even fathoms, in thickness, many of them consisting of laminated shale, inclosing the leaves of ferns and other terrestrial plants. We have also to explain the frequent intercalations of root beds, and the interposition here and there of brackish and marine deposits, demonstrating the occasional presence of the neighbouring sea. But these forest-covered deltas could only have been formed at the termination of large hydrographical basins, each drained by a great river and its tributaries; and the accumulation of sediment bears testimony to contemporaneous denudation on a large scale, and, therefore, to a wide area of land, probably containing within it one or more mountain chains.

In the case of the great Ohio or Appalachian coal field, the largest in the world, it seems clear that the uplands drained by one or more great rivers were chiefly to the eastward, or they occupied a space now filled by part of the Atlantic Ocean, for the mechanical deposits of mud and sand increase greatly in thickness and coarseness of material as we approach the eastern borders of the coal field, or the southeast flanks of the Allegheny Mountains, near Philadelphia. In that region numerous beds of pebbles, often of the size of a hen's egg, are seen to alternate with beds of pure coal.

But the American coal fields are all comprised within the 30th and 50th degrees of north latitude; and there is no reason to presume that the lands at the borders of which they originated ever penetrated so far or in such masses into the colder and arctic regions as to generate a cold climate. In the southern hemisphere, where the predominance of sea over land is now the distinguishing geographical feature, we nevertheless find a large part of the continent of Australia, as well as New Zealand, placed between the 30th and 50th degrees of south latitude. The two islands of New Zealand, taken together, are between 800 and 900 miles in length, with a breadth in some parts of ninety miles, and they stretch as far south as the 46th degree of latitude. They afford, therefore, a wide area for the growth of a terrestrial vegetation, and the botany of this region is characterised by abundance of ferns, one hundred and forty species of which are already known, some of them attaining the size of trees. In this respect the southern shores of New Zealand in the 46th degree of latitude almost vie with tropical islands. Another point of resemblance between the Flora of New Zealand and that of the ancient Carboniferous period is the prevalence of the fir tribe or of coniferous wood.

An argument of some weight in corroboration of the theory above explained respecting the geographical condition of the temperate and arctic latitudes of the northern hemisphere in the Carboniferous period may also be derived from an examination of those groups of strata which immediately preceded the coal. The fossils of the Devonian and Silurian strata in Europe and North America have led to the conclusion that they were formed for the most part in deep seas, far from land. In those older strata land plants are almost as rare as they are abundant or universal in

the coal measures. Those ancient deposits, therefore, may be supposed to have belonged to an epoch when dry land had only just begun to be upraised from the deep; a theory which would imply the existence during the Carboniferous epoch of islands, instead of an extensive continent, in the area where the coal was formed.

Such a state of things prevailing in the north, from the pole to the 30th parallel of latitude, if not neutralized by circumstances of a contrary tendency in corresponding regions south of the line, would give rise to a general warmth and uniformity of climate throughout the globe.

Changes in physical geography between the formation of the carboniferous strata and the chalk.—We have evidence in England that the strata of the ancient carboniferous group, already adverted to, were, in many instances, fractured and contorted, and often thrown into a vertical position, before the deposition of some even of the oldest known secondary rocks, such as the new red sandstone.

A freshwater deposit, called the Wealden, occurs in the upper part of the secondary series of the south of England, which, by its extent and fossils, attests the existence in that region of a large river draining a continent or island of considerable dimensions. We know that this land was clothed with wood and inhabited by huge terrestrial reptiles and birds. Its position so far to the north as the counties of Surrey and Sussex, at a time when the mean temperature of the climate is supposed to have been much hotter than at present, may at first sight appear inconsistent with the theory before explained, that the heat was caused by the gathering together of all the great masses of land in low latitudes, while the northern regions were almost entirely sea. But it must not be taken for granted that the geographical conditions already described (see Fig. 3) as capable of producing the extreme of heat were ever combined at any geological period of which we have as yet obtained information. It is more probable, from what has been stated in the preceding chapters, that a slight approximation to such an extreme state of things would be sufficient; in other words, if most of the dry land were tropical, and scarcely any of it arctic or antarctic, a prodigious elevation of temperature must ensue, even though a part of some continents should penetrate far into the temperate zones.

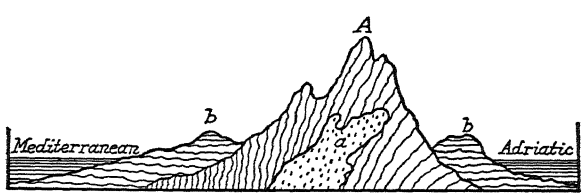
Changes during the tertiary periods.—The secondary and tertiary formations of Europe, when considered separately, may be contrasted as having very different characters; the secondary appearing to have been deposited in open seas, the tertiary in regions where dry land, lakes, bays, and perhaps inland seas, abounded. The secondary series is almost exclusively marine; the tertiary, even the oldest part, contains lacustrine strata, and not unfrequently freshwater and marine beds alternating. In fact, there is evidence of important geographical changes having occurred between the deposition of the cretaceous system, or uppermost of the secondary series, and that of the oldest tertiary group, and still more between the era of the latter and that of the newer tertiary formations. This change in the physical geography of Europe and North America was ac-

companied by an alteration no less remarkable in organic life, scarcely any *species* being common both to the secondary and tertiary rocks, and the fossils of the latter affording evidence of a different climate.

On the other hand, when we compare the tertiary formations of successive ages, we trace a gradual approximation in the imbedded fossils, from an assemblage in which extinct species predominate to one where the species agree for the most part with those now existing. In other words, we find a gradual increase of animals and plants fitted for our present climates, in proportion as the strata which we examine are more modern. Now, during all these successive tertiary periods, there are signs of a great increase of land in European and North American latitudes. By reference to the map (pages 294 and 295), and its description, the reader will see that about two thirds of the present European lands have emerged since the earliest tertiary group originated. Nor is this the only revolution which the same region has undergone within the period alluded to, some tracts which were previously land having gained in altitude, others, on the contrary, having sunk below their former level.

That the existing lands were not all upheaved at once into their present position is proved by the most striking evidence. Several Italian geologists, even before the time of Brocchi, had justly inferred that the Apennines were elevated several thousand feet above the level of the Mediterranean before the deposition of the modern Sub-Apennine beds which flank them on either side. What now constitutes the central calcareous chain of the Apennines must for a long time have been a narrow ridgy peninsula, branching off, at its northern extremity, from the Alps near Savona. This peninsula has since been raised from one to two thousand feet, by which movement the ancient shores, and, for a certain extent, the bed of the contiguous sea, have been laid dry, both on the side of the Mediterranean and the Adriatic.

The nature of these vicissitudes will be explained by the accompanying diagram, which represents a transverse section across the Italian peninsula. The inclined strata A are the disturbed formations of the Apennines into which the ancient igneous rocks *a* are supposed to have intruded themselves. At a lower level on each flank of the chain are the more recent shelly beds *b b,* which often contain rounded pebbles derived from the waste of contiguous parts of the older Apennine limestone. These, it will be seen, are horizontal, and lie in what is termed "unconformable stratification" on the more ancient series. They now constitute a line of



hills of moderate elevation between the sea and the Apennines, but never penetrate to the higher and more ancient valleys of that chain.

The remarkable break between the most modern of the known secondary rocks and the oldest tertiary may be apparent only, and ascribable to the present deficiency of our information. Already the marls and greensand of Heers near Tongres, in Belgium, observed by M. Dumont, and the "pisolitic limestone" of the neighbourhood of Paris, both intermediate in age between the Maestricht chalk and the lower Eocene strata, begin to afford us signs of a passage from one state of things to another. Nevertheless, it is far from impossible that the interval between the chalk and tertiary formations constituted an era in the earth's history, when the transition from one class of organic beings to another was, comparatively speaking, rapid. For if the doctrines above explained in regard to vicissitudes of temperature are sound, it will follow that changes of equal magnitude in the geographical features of the globe may at different periods produce very unequal effects on climate; and, so far as the existence of certain animals and plants depends on climate, the duration of species would be shortened or protracted, according to the rate at which the change of temperature proceeded.

Map showing the extent of surface in Europe which has at one period or another been covered by the sea since the commencement of the deposition of the older or Eocene tertiary strata.—The aforesaid map on pp. 294 and 295 will enable the reader to perceive at a glance the great extent of change of the physical geography of Europe, which can be proved to have taken place since some of the older tertiary strata began to be deposited. The proofs of submergence, during some part or other of this period, in all the dotted portions of the map, are of a most unequivocal character; for the area thus described is now covered by deposits containing the fossil remains of animals which could only have lived in salt water. The most ancient part of the period referred to cannot be deemed very remote, considered geologically; because the deposits of the Paris and London basins, and many other districts belonging to the older tertiary epoch, are newer than the greater part of the sedimentary rocks (those commonly called secondary and primary fossiliferous or paleozoic) of which the crust of the globe is composed. The species, moreover, of marine testacea, of which the remains are found in these older tertiary formations, are not entirely distinct from such as now live. Yet, notwithstanding the comparatively recent epoch to which this retrospect is carried, the variations in the distribution of land and sea depicted on the map form only a part of those which must have taken place during the period under consideration. Some approximation has merely been made to an estimate of the amount of *sea converted into land* in parts of Europe best known to geologists; but we cannot determine how much land has become sea during the same period; and there may have been repeated interchanges of land and water in the same places, changes of which no account is taken in the map, and respecting the amount of which little accurate information can ever be obtained.

I have extended the sea in some instances beyond the limits of the land now covered by tertiary formations and marine drift, because other geological data have been obtained for inferring the submergence of these tracts after the deposition of the Eocene strata had begun. Thus, for example, there are good reasons for concluding that part of the chalk of England (the North and South Downs, for example, together with the intervening secondary tracts) continued beneath the sea until the oldest tertiary beds had begun to accumulate.

A strait of the sea separating England and Wales has also been introduced, on the evidence afforded by shells of existing species found in a deposit of gravel, sand, loam, and clay, called the northern drift, by Sir R. Murchison. And Mr. Trimmer has discovered similar recent marine shells on the northern coast of North Wales, and on Moel Tryfane, near the Menai Straits, at the height of 1392 feet above the level of the sea!

Some raised sea beaches, and drift containing marine shells, which I examined in 1843, between Limerick and Dublin, and which have been traced over other parts of Ireland by different geologists, have required an extension of the dotted portions so as to divide that island into several. In improving this part of my map I have been especially indebted to the assistance of Mr. Oldham, who in 1843 announced to the British Association at Cork the fact that at the period when the drift or glacial beds were deposited, Ireland must have formed an archipelago such as is here depicted. A considerable part of Scotland might also have been represented in a similar manner as under water when the drift originated.

I was anxious, even in the title of this map, to guard the reader against the supposition that it was intended to represent the state of the physical geography of part of Europe at any *one point of time*. The difficulty, or rather the impossibility, of restoring the geography of the globe as it may have existed at any former period, especially a remote one, consists in this, that we can only point out where part of the sea has been turned into land, and are almost always unable to determine what land may have become sea. All maps, therefore, pretending to represent the geography of remote geological epochs must be ideal.

It may be proper to remark that the vertical movements to which the land is subject in certain regions occasion alternately the subsidence and the uprising of the surface; and that, by such oscillations at successive periods, a great area may have been entirely covered with marine deposits, although the whole may never have been beneath the waters at one time; nay, even though the relative proportion of land and sea may have continued unaltered throughout the whole period. I believe, however, that since the commencement of the tertiary period, the dry land in the northern hemisphere has been continually on the increase, both because it is now greatly in excess beyond the average proportion which land generally bears to water on the globe, and because a comparison of the secondary and tertiary strata affords indications, as I have already shown, of a passage from the condition of an ocean interspersed with islands to that of a large continent.

But supposing it were possible to represent all the vicissitudes in the distribution of land and sea that have occurred during the tertiary period, and to exhibit not only the actual existence of land where there was once sea, but also the extent of surface now submerged which may once have been land, the map would still fail to express all the important revolutions in physical geography which have taken place within the epoch under consideration. For the oscillations of level, as was before stated, have not merely been such as to lift up the land from below the water, but in some cases to occasion a rise of many thousand feet above the sea. Thus the Alps have acquired an additional altitude of 4000, and even in some places 10,000 feet; and the Apennines owe a considerable part of their present height to subterranean convulsions which have happened within the tertiary epoch.

Concluding remarks on changes in physical geography.—The foregoing observations, it may be said, are confined chiefly to Europe, and therefore merely establish the increase of dry land in a space which constitutes but a small portion of the northern hemisphere; but it was stated in the preceding chapter that the great Lowland of Siberia, lying chiefly between the latitudes 55° and 75° north (an area nearly equal to all Europe), is covered for the most part by marine strata, which, from the account given by Pallas, and more recently by Sir R. Murchison, belongs to a period when all or nearly all the shells were of a species still living in the north. The emergence, therefore, of this area from the deep is, comparatively speaking, a very modern event, and must, as before remarked, have caused a great increase of cold throughout the globe.

Upon a review, then, of all the facts above enumerated, respecting the ancient geography of the globe as attested by geological monuments, there appear good grounds for inferring that changes of climate coincided with remarkable revolutions in the former position of sea and land. A wide expanse of ocean, interspersed with islands, seems to have pervaded the northern hemisphere at the periods when the Silurian and carboniferous rocks were formed, and a warm and very uniform temperature then prevailed. Subsequent modifications in climate accompanied the deposition of the secondary formations, when repeated changes were effected in the physical geography of our northern latitudes. Lastly, the refrigeration became most decided, and the climate most nearly assimilated to that now enjoyed, when the lands in Europe and northern Asia had attained their full extension, and the mountain chains their actual height.

VI. Supposed Intensity of Aqueous Forces at Remote Periods

Intensity of aqueous causes.—The great problem considered in the preceding chapters, namely, whether the former changes of the earth made known to us by geology resemble in kind and degree those now in daily

progress, may still be contemplated from several other points of view. We may inquire, for example, whether there are any grounds for the belief entertained by many that the intensity both of aqueous and of igneous forces, in remote ages, far exceeded that which we witness in our own times.

First, then, as to aqueous causes: it has been shown in our history of the science that Woodward did not hesitate, in 1695, to teach that the entire mass of fossiliferous strata contained in the earth's crust had been deposited in a few months; and, consequently, as their mechanical and derivative origin was already admitted, the reduction of rocky masses into mud, sand, and pebbles, the transportation of the same to a distance, and their accumulation elsewhere in regular strata were all assumed to have taken place with a rapidity unparalleled in modern times. This doctrine was modified by degrees, in proportion as different classes of organic remains, such as shells, corals, and fossil plants, had been studied with attention. Analogy led every naturalist to assume that each full-grown individual of the animal or vegetable kingdom had required a certain number of months or years for the attainment of maturity and the perpetuation of its species by generation; and thus the first approach was made to the conception of a common standard of time, without which there are no means whatever of measuring the comparative rate at which any succession of events has taken place at two distinct periods. This standard consisted of the average duration of the lives of individuals of the same genera or families in the animal and vegetable kingdoms; and the multitude of fossils dispersed through successive strata implied the continuance of the same species for many generations. At length the idea that species themselves had had a limited duration arose out of the observed fact that sets of strata of different ages contained fossils of distinct species. Finally, the opinion became general that in the course of ages one assemblage of animals and plants had disappeared after another again and again, and new tribes had started into life to replace them.

Denudation.—In addition to the proofs derived from organic remains, the forms of stratification led also, on a fuller investigation, to the belief that sedimentary rocks had been slowly deposited; but it was still supposed that *denudation,* or the power of running water, and the waves and currents of the ocean, to strip off superior strata and lay bare the rocks below, had formerly operated with an energy wholly unequalled in our times. These opinions were both illogical and inconsistent, because deposition and denudation are parts of the same process, and what is true of the one must be true of the other. Their speed must be always limited by the same causes, and the conveyance of solid matter to a particular region can only keep pace with its removal from another, so that the aggregate of sedimentary strata in the earth's crust can never exceed in volume the amount of solid matter which has been ground down and washed away by running water. How vast then must be the spaces which this abstraction of matter has left vacant! How far exceeding in dimensions all the valleys, however numerous, and the hollows, however vast,

which we can prove to have been cleared out by aqueous erosion! The evidences of the work of denudation are defective, because it is the nature of every destroying cause to obliterate the signs of its own agency; but the amount of reproduction in the form of sedimentary strata must always afford a true measure of the minimum of denudation which the earth's surface has undergone.

Supposed universality of ancient deposits.—The next fallacy which has helped to perpetuate the doctrine that the operations of water were on a different and grander scale in ancient times is founded on the indefinite areas over which homogeneous deposits were supposed to extend. No modern sedimentary strata, it is said, equally identical in mineral character and fossil contents, can be traced continuously from one quarter of the globe to another. But the first propagators of these opinions were very slightly acquainted with the inconstancy in mineral composition of the ancient formations, and equally so of the wide spaces over which the same kind of sediment is now actually distributed by rivers and currents in the course of centuries. The persistency of character in the older series was exaggerated, its extreme variability in the newer was assumed without proof.

In regard to the imagined universality of particular rocks of ancient date, it was almost unavoidable that this notion, when once embraced, should be perpetuated; for the same kinds of rock have occasionally been reproduced at successive epochs: and when once the agreement or disagreement in mineral character alone was relied on as the test of age, it followed that similar rocks, if found even at the antipodes, were referred to the same era, until the contrary could be shown.

Now it is usually impossible to combat such an assumption on geological grounds, so long as we are imperfectly acquainted with the order of superposition and the organic remains of these same formations. Thus, for example, a group of red marl and red sandstone, containing salt and gypsum, being interposed in England between the Lias and the Coal, all other red marls and sandstones, associated some of them with salt, and others with gypsum, and occurring not only in different parts of Europe, but in North America, Peru, India, the salt deserts of Asia, those of Africa —in a word, in every quarter of the globe—were referred to one and the same period. The burden of proof was not supposed to rest with those who insisted on the identity in age of all these groups—their identity in mineral composition was thought sufficient. It was in vain to urge as an objection the improbability of the hypothesis which implies that all the moving waters on the globe were once simultaneously charged with sediment of a red colour.

But the rashness of pretending to identify, in age, all the red sandstones and marls in question has at length been sufficiently exposed by the discovery that, even in Europe, they belong decidedly to many different epochs. It is already ascertained that the red sandstone and red marl containing the rock salt of Cardona in Catalonia is newer than the Oolitic, if not more modern than the Cretaceous period. It is also known that cer-

tain red marls and variegated sandstones in Auvergne which are undistinguishable in mineral composition from the New Red Sandstone of English geologists belong, nevertheless, to the Eocene period: and, lastly, the gypseous red marl of Aix, in Provence, formerly supposed to be a marine secondary group, is now acknowledged to be a tertiary freshwater formation. In Nova Scotia one great deposit of red marl, sandstone, and gypsum, precisely resembling in mineral character the "New Red" of England, occurs as a member of the carboniferous group, and in the United States, near the Falls of Niagara, a similar formation constitutes a subdivision of the Silurian series.

VII. On the Supposed Former Intensity of the Igneous Forces

WHEN REASONING on the intensity of volcanic action at former periods, as well as on the power of moving water, already treated of, geologists have been ever prone to represent Nature as having been prodigal of violence and parsimonious of time. Now, although it is less easy to determine the relative ages of the volcanic than of the fossiliferous formations, it is undeniable that igneous rocks have been produced at all geological periods, or as often as we find distinct deposits marked by peculiar animal and vegetable remains. It can be shown that rocks commonly called trappean have been injected into fissures and ejected at the surface, both before and during the deposition of the carboniferous series, and at the time when the Magnesian Limestone and when the Upper New Red Sandstone were formed, or when the Lias, Oolite, Greensand, Chalk and the several tertiary groups newer than the chalk originated in succession. Nor is this all; distinct volcanic products may be referred to the subordinate divisions of each period, such as the Carboniferous, as in the county of Fife, in Scotland, where certain masses of contemporaneous trap are associated with the Lower, others with the Upper Coal Measures. And if one of these masses is more minutely examined, we find it to consist of the products of a great many successive outbursts, by which scoriæ and lava were again and again emitted, and afterwards consolidated, then fissured, and finally traversed by melted matter constituting what are called dikes. As we enlarge, therefore, our knowledge of the ancient rocks formed by subterranean heat, we find ourselves compelled to regard them as the aggregate effects of innumerable eruptions, each of which may have been comparable in violence to those now experienced in volcanic regions.

Gradual development of subterranean movements.—The extreme violence of the subterranean forces in remote ages has been often inferred from the facts that the older rocks are more fractured and dislocated than the newer. But what other result could we have anticipated if the quantity of movement had been always equal in equal periods of time? Time must, in that case, multiply the derangement of strata in the ratio

of their antiquity. Indeed the numerous exceptions to the above rule which we find in nature present at first sight the only objection to the hypothesis of uniformity. For the more ancient formations remain in many places horizontal, while in others much newer strata are curved and vertical.

That the more impressive effects of subterranean power, such as the upheaval of mountain chains, may have been due to multiplied convulsions of moderate intensity rather than to a few paroxysmal explosions will appear the less improbable when the gradual and intermittent development of volcanic eruptions in times past is once established. It is now very generally conceded that these eruptions have their source in the same causes as those which give rise to the permanent elevation and sinking of land; the admission, therefore, that one of the two volcanic or subterranean processes has gone on gradually draws with it the conclusion that the effects of the other have been elaborated by successive and gradual efforts.

Faults.—The same reasoning is applicable to great *faults,* or those striking instances of the upthrow or downthrow of large masses of rock, which have been thought by some to imply tremendous catastrophes wholly foreign to the ordinary course of nature. Thus we have in England faults in which the vertical displacement is between 600 and 3000 feet and the horizontal extent thirty miles or more, the width of the fissures since filled up with rubbish varying from ten to fifty feet. But when we inquire into the proofs of the mass having risen or fallen suddenly on the one side of these great rents, several hundreds or thousands of feet above or below the rock with which it was once continuous on the other side, we find the evidence defective. There are grooves, it is said, and scratches on the rubbed and polished walls, which have often one common direction, favouring the theory that the movement was accomplished by a single stroke and not by a series of interrupted movements. But, in fact, the striæ are not always parallel in such cases, but often irregular, and sometimes the stones and earth which are in the middle of the fault, or fissure, have been polished and striated by friction in different directions, showing that there have been slidings subsequent to the first introduction of the fragmentary matter. Nor should we forget that the last movement must always tend to obliterate the signs of previous trituration, so that neither its instantaneousness nor the uniformity of its direction can be inferred from the parallelism of the striæ that have been last produced.

When rocks have been once fractured, and freedom of motion communicated to detached portions of them, these will naturally continue to yield in the same direction, if the process of upheaval or of undermining be repeated again and again. The incumbent mass will always give way along the lines of least resistance, or where it was formerly rent asunder. Probably, the effects of reiterated movement, whether upward or downward, in a fault, may be undistinguishable from those of a single and instantaneous rise or subsidence; and the same may be said of the rising or

falling of continental masses, such as Sweden or Greenland, which we know to take place slowly and insensibly.

Slow upheaval and subsidence.—Recent observations have disclosed to us the wonderful fact that not only the west coast of South America, but also other large areas, some of them several thousand miles in circumference, such as Scandinavia, and certain archipelagos in the Pacific, are slowly and insensibly rising; while other regions, such as Greenland and parts of the Pacific and Indian Oceans, in which atolls or circular coral islands abound, are as gradually sinking. That all the existing continents and submarine abysses may have originated in movements of this kind, continued throughout incalculable periods of time, is undeniable, and the denudation which the dry land appears everywhere to have suffered favours the idea that it was raised from the deep by a succession of upward movements, prolonged throughout indefinite periods. For the action of waves and currents on land slowly emerging from the deep affords the only power by which we can conceive so many deep valleys and wide spaces to have been denuded as those which are unquestionably the effects of running water.

But perhaps it may be said that there is no analogy between the slow upheaval of broad plains or table lands and the manner in which we must presume all mountain chains, with their inclined strata, to have originated. It seems, however, that the Andes have been rising century after century, at the rate of several feet, while the Pampas on the east have been raised only a few inches in the same time. Crossing from the Atlantic to the Pacific, in a line passing through Mendoza, Mr. Darwin traversed a plain 800 miles broad, the eastern part of which has emerged from beneath the sea at a very modern period. The slope from the Atlantic is at first very gentle, then greater, until the traveller finds, on reaching Mendoza, that he has gained, almost insensibly, a height of 4000 feet. The mountainous district then begins suddenly, and its breadth from Mendoza to the shores of the Pacific is 120 miles, the average height of the principal chain being from 15,000 to 16,000 feet, without including some prominent peaks, which ascend much higher. Now all we require, to explain the origin of the principal inequalities of level here described, is to imagine, first, a zone of more violent movement to the west of Mendoza, and, secondly, to the east of that place, an upheaving force, which died away gradually as it approached the Atlantic. In short, we are only called upon to conceive that the region of the Andes was pushed up four feet in the same period in which the Pampas near Mendoza rose one foot and the plains near the shores of the Atlantic one inch. In Europe we have learnt that the land at the North Cape ascends about five feet in a century, while farther to the south the movements diminish in quantity first to a foot, and then, at Stockholm, to three inches in a century, while at certain points still farther south there is no movement.

In conclusion, I may observe that one of the soundest objections to the theory of the sudden upthrow or downthrow of mountain chains is this, that it provides us with too much force of one kind, namely, that of

subterranean movement, while it deprives us of another kind of mechanical force, namely, that exerted by the waves and currents of the ocean, which the geologist requires for the denudation of land during its slow upheaval or depression. It may be safely affirmed that the quantity of igneous and aqueous action—of volcanic eruption and denudation—of subterranean movement and sedimentary deposition—not only of past ages, but of one geological epoch, or even the fraction of an epoch, has exceeded immeasurably all the fluctuations of the inorganic world which have been witnessed by man. But we have still to inquire whether the time to which each chapter or page or paragraph of the earth's autobiography relates was not equally immense when contrasted with a brief era of 3000 or 5000 years. The real point on which the whole controversy turns is the relative amount of work done by mechanical force in given quantities of time, past and present. Before we can determine the relative intensity of the force employed, we must have some fixed standard by which to measure the time expended in its development at two distinct periods. It is not the magnitude of the effects, however gigantic their proportions, which can inform us in the slightest degree whether the operation was sudden or gradual, insensible or paroxysmal. It must be shown that a slow process could never in any series of ages give rise to the same results.

The advocate of paroxysmal energy might assume an uniform and fixed rate of variation in times past and present for the animate world, that is to say, for the dying-out and coming-in of species, and then endeavour to prove that the changes of the inanimate world have not gone on in a corresponding ratio. But the adoption of such a standard of comparison would lead, I suspect, to a theory by no means favourable to the pristine intensity of natural causes. That the present state of the organic world is not stationary can be fairly inferred from the fact that some species are known to have become extinct in the course even of the last three centuries, and that the exterminating causes always in activity, both on the land and in the waters, are very numerous; also, because man himself is an extremely modern creation; and we may therefore reasonably suppose that some of the mammalia now contemporary with man, as well as a variety of species of inferior classes, may have been recently introduced into the earth, to supply the places of plants and animals which have from time to time disappeared. But granting that some such secular variation in the zoological and botanical worlds is going on, and is by no means wholly inappreciable to the naturalist, still it is certainly far less manifest than the revolution always in progress in the inorganic world. Every year some volcanic eruptions take place, and a rude estimate might be made of the number of cubic feet of lava and scoriæ poured or cast out of various craters. The amount of mud and sand deposited in deltas, and the advance of new land upon the sea, or the annual retreat of wasting sea cliffs, are changes the minimum amount of which might be roughly estimated. The quantity of land raised above or depressed below the level of the sea might also be computed, and the change arising from such

movements in a century might be conjectured. Suppose the average rise of the land in some parts of Scandinavia to be as much as five feet in a hundred years, the present seacoast might be uplifted 700 feet in fourteen thousand years; but we should have no reason to anticipate, from any zoological data hitherto acquired, that the molluscous fauna of the northern seas would in that lapse of years undergo any sensible amount of variation. We discover sea beaches in Norway 700 feet high, in which the shells are identical with those now inhabiting the German Ocean; for the rise of land in Scandinavia, however insensible to the inhabitants, has evidently been rapid when compared to the rate of contemporaneous change in the testaceous fauna of the German Ocean. Were we to wait therefore until the mollusca shall have undergone as much fluctuation as they underwent between the period of the Lias and the Upper Oolite formations, or between the Oolite and Chalk, nay, even between any two of eight subdivisions of the Eocene series, what stupendous revolutions in physical geography ought we not to expect, and how many mountain chains might not be produced by the repetition of shocks of moderate violence, or by movements not even perceptible by man!

Or, if we turn from the mollusca to the vegetable kingdom, and ask the botanist how many earthquakes and volcanic eruptions might be expected, and how much the relative level of land and sea might be altered, or how far the principal deltas will encroach upon the ocean, or the sea cliffs recede from the present shores, before the species of European forest trees will die out, he would reply that such alterations in the inanimate world might be multiplied indefinitely before he should have reason to anticipate, by reference to any known data, that the existing species of trees in our forests would disappear and give place to others. In a word, the movement of the inorganic world is obvious and palpable, and might be likened to the minute hand of a clock, the progress of which can be seen and heard, whereas the fluctuations of the living creation are nearly invisible and resemble the motion of the hour hand of a timepiece. It is only by watching it attentively for some time, and comparing its relative position after an interval, that we can prove the reality of its motion.

VIII. Uniformity in the Series of Past Changes in the Animate and Inanimate World

Origin of the doctrine of alternate periods of repose and disorder.— It has been truly observed that when we arrange the fossiliferous formations in chronological order, they constitute a broken and defective series of monuments: we pass without any intermediate gradations from systems of strata which are horizontal to other systems which are highly inclined, from rocks of peculiar mineral composition to others which have a

character wholly distinct—from one assemblage of organic remains to another, in which frequently all the species, and most of the genera, are different. These violations of continuity are so common as to constitute the rule rather than the exception, and they have been considered by many geologists as conclusive in favour of sudden revolutions in the inanimate and animate world. According to the speculations of some writers, there have been in the past history of the planet alternate periods of tranquillity and convulsion, the former enduring for ages, and resembling that state of things now experienced by man: the other brief, transient, and paroxysmal, giving rise to new mountains, seas, and valleys, annihilating one set of organic beings and ushering in the creation of another.

It is true that in the solid framework of the globe, we have a chronological chain of natural records, and that many links in this chain are wanting; but a careful consideration of all the phenomena will lead to the opinion that the series was originally defective—that it has been rendered still more so by time—that a great part of what remains is inaccessible to man, and even of that fraction which is accessible, nine tenths are to this day unexplored.

How the facts may be explained by assuming a uniform series of changes.—The readiest way, perhaps, of persuading the reader that we may dispense with great and sudden revolutions in the geological order of events is by showing him how a regular and uninterrupted series of changes in the animate and inanimate world may give rise to such breaks in the sequence, and such unconformability of stratified rocks, as are usually thought to imply convulsions and catastrophes. It is scarcely necessary to state that the order of events thus assumed to occur, for the sake of illustration, must be in harmony with all the conclusions legitimately drawn by geologists from the structure of the earth, and must be equally in accordance with the changes observed by man to be now going on in the living as well as in the inorganic creation. It may be necessary in the present state of science to supply some part of the assumed course of nature hypothetically; but if so, this must be done without any violation of probability, and always consistently with the analogy of what is known both of the past and present economy of our system.

UNIFORMITY OF CHANGE CONSIDERED FIRST IN REFERENCE TO THE LIVING CREATION

First, in regard to the vicissitudes of the living creation, all are agreed that the sedimentary strata found in the earth's crust are divisible into a variety of groups, more or less dissimilar in their organic remains and mineral composition. The conclusion universally drawn from the study and comparison of these fossiliferous groups is this, that at successive periods, distinct tribes of animals and plants have inhabited the land and waters, and that the organic types of the newer formations are more analogous to species now existing than those of more ancient rocks. If we

then turn to the present state of the animate creation, and inquire whether it has now become fixed and stationary, we discover that, on the contrary, it is in a state of continual flux—that there are many causes in action which tend to the extinction of species, and which are conclusive against the doctrine of their unlimited durability. But natural history has been successfully cultivated for so short a period that a few examples only of local, and perhaps but one or two of absolute, extirpation can as yet be proved, and these only where the interference of man has been conspicuous. It will nevertheless appear evident that man is not the only exterminating agent; and that, independently of his intervention, the annihilation of species is promoted by the multiplication and gradual diffusion of every animal or plant.

Recent origin of man, and gradual approach in the tertiary fossils of successive periods from an extinct to the recent fauna.—The geologist, however, if required to advance some fact which may lend countenance to the opinion that in the most modern times, that is to say, after the greater part of the existing fauna and flora were established on the earth, there has still been a new species superadded, may point to man himself as furnishing the required illustration—for man must be regarded by the geologist as a creature of yesterday, not merely in reference to the past history of the organic world, but also in relation to that particular state of the animate creation of which he forms a part. The comparatively modern introduction of the human race is proved by the absence of the remains of man and his works, not only from all strata containing a certain proportion of fossil shells of extinct species, but even from a large part of the newest strata, in which all the fossil individuals are referable to species still living.

To enable the reader to appreciate the full force of this evidence I shall give a slight sketch of the information obtained from the newer strata, respecting fluctuations in the animate world in times immediately antecedent to the appearance of man.

In tracing the series of fossiliferous formations from the more ancient to the more modern, the first deposits in which we meet with assemblages of organic remains, having a near analogy to the fauna of certain parts of the globe in our own time, are those commonly called tertiary. Even in the Eocene, or oldest subdivision of these tertiary formations, some few of the testacea belong to existing species, although almost all of them, and apparently all the associated vertebrata, are now extinct. These Eocene strata are succeeded by a great number of more modern deposits, which depart gradually in the character of their fossils from the Eocene type, and approach more and more to that of the living creation. In the present state of science, it is chiefly by the aid of shells that we are enabled to arrive at these results, for of all classes the testacea are the most generally diffused in a fossil state, and may be called the medals principally employed by nature, in recording the chronology of past events. In the Miocene deposits, which are next in succession to the Eocene, we begin to find a considerable number, although still a minority, of recent species,

intermixed with some fossils common to the preceding epoch. We then arrive at the Pliocene strata, in which species now contemporary with man begin to preponderate, and in the newest of which nine tenths of the fossils agree with species still inhabiting the neighbouring sea.

In thus passing from the older to the newer members of the tertiary system we meet with many chasms, but none which separate entirely, by a broad line of demarcation, one state of the organic world from another. There are no signs of an abrupt termination of one fauna and flora, and the starting into life of new and wholly distinct forms. Although we are far from being able to demonstrate geologically an insensible transition from the Eocene to the Miocene, or even from the latter to the recent fauna, yet the more we enlarge and perfect our general survey, the more nearly do we approximate to such a continuous series, and the more gradually are we conducted from times when many of the genera and nearly all the species were extinct to those in which scarcely a single species flourished which we do not know to exist at present.

It had often been objected that the evidence of fossil species occurring in two consecutive formations was confined to the testacea or zoophytes, the characters of which are less marked and decisive than those afforded by the vertebrate animals. But Mr. Owen has lately insisted on the important fact that not a few of the quadrupeds which now inhabit our island, and among others the horse, the ass, the hog, the smaller wild ox, the goat, the red deer, the roe, the beaver, and many of the diminutive rodents, are the same as those which once co-existed with the mammoth, the great northern hippopotamus, two kinds of rhinoceros, and other mammalia long since extinct. "A part," he observes, "and not the whole of the modern tertiary fauna has perished, and hence we may conclude that the cause of their destruction has not been a violent and universal catastrophe from which none could escape."

Had we discovered evidence that man had come into the earth at a period as early as that when a large number of the fossil quadrupeds now living, and almost all the recent species of land, freshwater, and marine shells were in existence, we should have been compelled to ascribe a much higher antiquity to our species than even the boldest speculations of the ethnologist require, for no small part of the great physical revolution depicted on the map of Europe before described took place very gradually after the recent testacea abounded almost to the exclusion of the extinct. Thus, for example, in the deposits called the "northern drift," or the glacial formation of Europe and North America, the fossil marine shells can easily be identified with species either now inhabiting the neighbouring sea or living in the seas of higher latitudes. Yet they exhibit no memorials of the human race, or of articles fabricated by the hand of man.

There are other post-tertiary formations of fluviatile origin, in the centre of Europe, in which the absence of human remains is perhaps still more striking, because, when formed, they must have been surrounded by dry land. I allude to the silt or *loess* of the basin of the Rhine, which must have gradually filled up the great valley of that river since the time when

its waters, and the contiguous lands, were inhabited by the existing species of freshwater and terrestrial mollusks. Showers of ashes, thrown out by some of the last eruptions of the Eifel volcanos, fell during the deposition of this fluviatile silt, and were interstratified with it. But these volcanos became exhausted, the valley was re-excavated through the silt, and again reduced to its present form before the period of human history. The study, therefore, of this shelly silt reveals to us the history of a long series of events, which occurred after the testacea now living inhabited the land and rivers of Europe, and the whole terminated without any signs of the coming of man into that part of the globe.

To conclude, it appears that, in going back from the recent to the Eocene period, we are carried by many successive steps from the fauna now contemporary with man to an assemblage of fossil species wholly different from those now living. In this retrospect we have not yet succeeded in tracing back a perfect transition from the recent to an extinct fauna; but there are usually so many species in common to the groups which stand next in succession as to show that there is no great chasm, no signs of a crisis when one class of organic beings was annihilated to give place suddenly to another. This analogy, therefore, derived from a period of the earth's history which can best be compared with the present state of things, and more thoroughly investigated than any other, leads to the conclusion that the extinction and creation of species has been and is the result of a slow and gradual change in the organic world.

UNIFORMITY OF CHANGE CONSIDERED, SECONDLY, IN REFERENCE TO SUBTERRANEAN MOVEMENTS

Certain countries have, from time immemorial, been rudely shaken again and again, while others, comprising by far the largest part of the globe, have remained to all appearance motionless. In the regions of convulsion rocks have been rent asunder, the surface has been forced up into ridges, chasms have opened, or the ground throughout large spaces has been permanently lifted up above or let down below its former level. In the regions of tranquillity some areas have remained at rest, but others have been ascertained by a comparison of measurements, made at different periods, to have risen by an insensible motion, as in Sweden, or to have subsided very slowly, as in Greenland. That these same movements, whether ascending or descending, have continued for ages in the same direction has been established by geological evidence. Thus we find both on the east and west coast of Sweden that ground which formerly constituted the bottom of the Baltic and of the ocean has been lifted up to an elevation of several hundred feet above high-water mark. The rise within the historical period has not amounted to many yards, but the greater extent of antecedent upheaval is proved by the occurrence in inland spots, several hundred feet high, of deposits filled with fossil shells of species now living either in the ocean or the Baltic.

To detect proofs of slow and gradual subsidence must in general be more difficult; but the form of circular coral reefs and lagoon islands will satisfy the reader that there are spaces on the globe, several thousand miles in circumference, throughout which the downward movement has predominated for ages, and yet the land has never, in a single instance, gone down suddenly for several hundred feet at once. Yet geology demonstrates that the persistency of subterranean movements in one direction has not been perpetual throughout all past time. There have been great oscillations of level by which a surface of dry land has been submerged to a depth of several thousand feet, and then at a period long subsequent raised again and made to emerge. Nor have the regions now motionless been always at rest; and some of those which are at present the theatres of reiterated earthquakes have formerly enjoyed a long continuance of tranquillity. But although disturbances have ceased after having long prevailed, or have recommenced after a suspension for ages, there has been no universal disruption of the earth's crust or desolation of the surface since times the most remote. The non-occurrence of such a general convulsion is proved by the perfect horizontality now retained by some of the most ancient fossiliferous strata throughout wide areas.

Inferences derived from unconformable strata.—That the subterranean forces have visited different parts of the globe at successive periods is inferred chiefly from the unconformability of strata belonging to groups of different ages. Thus, for example, on the borders of Wales and Shropshire we find the slaty beds of the ancient Silurian system curved and vertical, while the beds of the overlying carboniferous shale and sandstone are horizontal. All are agreed that in such a case the older set of strata had suffered great dislocation before the deposition of the newer or carboniferous beds, and that these last have never since been convulsed by any movements of excessive violence. But the strata of the inferior group suffered only a local derangement, and rocks of the same age are by no means found everywhere in a curved or vertical position. In various parts of Europe, and particularly near Lake Wener in the south of Sweden, and in many parts of Russia, beds of the same Silurian system maintain the most perfect horizontality; and a similar observation may be made respecting limestones and shales of the like antiquity in the Great Lake District of Canada and the United States. They are still as flat and horizontal as when first formed; yet since their origin not only have most of the actual mountain chains been uplifted, but the very rocks of which those mountains are composed have been formed.

UNIFORMITY OF CHANGE CONSIDERED, THIRDLY, IN REFERENCE TO SEDIMENTARY DEPOSITION

If we survey the surface of the globe we immediately perceive that it is divisible into areas of deposition and non-deposition, or, in other words, at any given time there are spaces which are the recipients, others

which are not the recipients, of sedimentary matter. No new strata, for example, are thrown down on dry land, which remains the same from year to year; whereas, in many parts of the bottom of seas and lakes, mud, sand, and pebbles are annually spread out by rivers and currents. There are also great masses of limestone growing in some seas, or in mid-ocean, chiefly composed of corals and shells.

No sediment deposited on dry land.—As to the dry land, so far from being the receptacle of fresh accessions of matter, it is exposed almost everywhere to waste away. Forests may be as dense and lofty as those of Brazil, and may swarm with quadrupeds, birds, and insects, yet at the end of ten thousand years one layer of black mould, a few inches thick, may be the sole representative of those myriads of trees, leaves, flowers, and fruits, those innumerable bones and skeletons of birds, quadrupeds, and reptiles, which tenanted the fertile region. Should this land be at length submerged, the waves of the sea may wash away in a few hours the scanty covering of mould, and it may merely impart a darker shade of colour to the next stratum of marl, sand, or other matter newly thrown down. So also at the bottom of the ocean where no sediment is accumulating, seaweed, zoophytes, fish, and even shells, may multiply for ages and decompose, leaving no vestige of their form or substance behind. Their decay, in water, although more slow, is as certain and eventually as complete as in the open air. Nor can they be perpetuated for indefinite periods in a fossil state, unless imbedded in some matrix which is impervious to water or which at least does not allow a free percolation of that fluid, impregnated as it usually is, with a slight quantity of carbonic or other acid. Such a free percolation may be prevented either by the mineral nature of the matrix itself, or by the superposition of an impermeable stratum: but if unimpeded the fossil shell or bone will be dissolved and removed, particle after particle, and thus entirely effaced, unless petrifaction or the substitution of mineral for organic matter happen to take place.

That there has been land as well as sea at all former geological periods, we know from the fact that fossil trees and terrestrial plants are imbedded in rocks of every age. Occasionally lacustrine and fluviatile shells, insects, or the bones of amphibious or land reptiles point to the same conclusion. The existence of dry land at all periods of the past implies, as before mentioned, the partial deposition of sediment, or its limitation to certain areas; and the next point to which I shall call the reader's attention is the shifting of these areas from one region to another.

First, then, variations in the site of sedimentary deposition are brought about independently of subterranean movements. There is always a slight change from year to year, or from century to century. The sediment of the Rhone, for example, thrown into the Lake of Geneva, is now conveyed to a spot a mile and a half distant from that where it accumulated in the tenth century, and six miles from the point where the delta began originally to form. We may look forward to the period when this lake will be filled up, and then the distribution of the transported matter will be suddenly altered, for the mud and sand brought down from the

Alps will thenceforth, instead of being deposited near Geneva, be carried nearly 200 miles southwards, where the Rhone enters the Mediterranean.

But, secondly, all these causes of fluctuation in the sedimentary areas are entirely subordinate to those great upward or downward movements of land which have been already described as prevailing over large tracts of the globe. By such elevation or subsidence certain spaces are gradually submerged, or made gradually to emerge:—in the one case sedimentary deposition may be suddenly renewed after having been suspended for ages, in the other as suddenly made to cease after having continued for an indefinite period.

Causes of variation in mineral character of successive sedimentary groups.—If deposition be renewed after a long interval, the new strata will usually differ greatly from the sedimentary rocks previously formed in the same place, and especially if the older rocks have suffered derangement, which implies a change in the physical geography of the district since the previous conveyance of sediment to the same spot. It may happen, however, that, even when the inferior group is horizontal and conformable to the upper strata, these last may still differ entirely in mineral character, because since the origin of the older formation the geography of some distant country has been altered. In that country rocks before concealed may have become exposed by denudation; volcanos may have burst out and covered the surface with scoriæ and lava, or new lakes may have been formed by subsidence; and other fluctuations may have occurred, by which the materials brought down from thence by rivers to the sea have acquired a distinct mineral character.

It is well known that the stream of the Mississippi is charged with sediment of a different colour from that of the Arkansas and Red Rivers, which are tinged with red mud, derived from rocks of porphyry in "the far west." The waters of the Uruguay, says Darwin, draining a granitic country, are clear and black, those of the Parana, red. The mud with which the Indus is loaded, says Burnes, is of a clayey hue, that of the Chenab, on the other hand, is reddish, that of the Sutlej is more pale. The same causes which make these several rivers, sometimes situated at no great distance the one from the other, to differ greatly in the character of their sediment will make the waters draining the same country at different epochs, especially before and after great revolutions in physical geography, to be entirely dissimilar.

Why successive sedimentary groups contain distinct fossils.—If, in the next place, we assume, for reasons before stated, a continual extinction of species and introduction of others into the globe, it will then follow that the fossils of strata formed at two distant periods on the same spot will differ even more certainly than the mineral composition of the same. For rocks of the same kind have sometimes been reproduced in the same district after a long interval of time, whereas there are no facts leading to the opinion that species which have once died out have ever been reproduced. The submergence then of land must be often attended by the commencement of a new class of sedimentary deposits, characterized by a new

set of fossil animals and plants, while the reconversion of the bed of the sea into land may arrest at once and for an indefinite time the formation of geological monuments. Should the land again sink, strata will again be formed; but one or many entire revolutions in animal or vegetable life may have been completed in the interval.

Conditions requisite for the original completeness of a fossiliferous series.—If we infer, for reasons before explained, that fluctuations in the animate world are brought about by the slow and successive removal and creation of species, we shall be convinced that a rare combination of circumstances alone can give rise to such a series of strata as will bear testimony to a gradual passage from one state of organic life to another. To produce such strata nothing less will be requisite than the fortunate coincidence of the following conditions: first, a never-failing supply of sediment in the same region throughout a period of vast duration; secondly, the fitness of the deposit in every part for the permanent preservation of imbedded fossils; and, thirdly, a gradual subsidence to prevent the sea or lake from being filled up and converted into land.

In certain parts of the Pacific and Indian Oceans, most of these conditions, if not all, are complied with, and the constant growth of coral, keeping pace with the sinking of the bottom of the sea, seems to have gone on so slowly, for such indefinite periods, that the signs of a gradual change in organic life might probably be detected in that quarter of the globe, if we could explore its submarine geology. Instead of the growth of coralline limestone, let us suppose, in some other place, the continuous deposition of fluviatile mud and sand, such as the Ganges and Brahmapootra have poured for thousands of years into the Bay of Bengal. Part of this bay, although of considerable depth, might at length be filled up before an appreciable amount of change was effected in the fish, mollusca, and other inhabitants of the sea and neighbouring land. But, if the bottom be lowered by sinking at the same rate that it is raised by fluviatile mud, the bay can never be turned into dry land. In that case one new layer of matter may be superimposed upon another for a thickness of many thousand feet, and the fossils of the inferior beds may differ greatly from those entombed in the uppermost, yet every intermediate gradation may be indicated in the passage from an older to a newer assemblage of species. Granting, however, that such an unbroken sequence of monuments may thus be elaborated in certain parts of the sea, and that the strata happen to be all of them well adapted to preserve the included fossils from decomposition, how many accidents must still concur before these submarine formations will be laid open to our investigation! The whole deposit must first be raised several thousand feet, in order to bring into view the very foundation; and during the process of exposure the superior beds must not be entirely swept away by denudation.

In the first place, the chances are as three to one against the mere emergence of the mass above the waters, because three fourths of the globe are covered by the ocean. But if it be upheaved and made to constitute part of the dry land, it must also, before it can be available for our

instruction, become part of that area already surveyed by geologists; and this area comprehends perhaps less than a tenth of the whole earth. In this small fraction of land already explored, and still very imperfectly known, we are required to find a set of strata, originally of limited extent, and probably much lessened by subsequent denudation.

Yet it is precisely because we do not encounter at every step the evidence of such gradations from one state of the organic world to another that so many geologists embrace the doctrine of great and sudden revolutions in the history of the animate world. Not content with simply availing themselves, for the convenience of classification, of those gaps and chasms which here and there interrupt the continuity of the chronological series, as at present known, they deduce, from the frequency of these breaks in the chain of records, an irregular mode of succession in the events themselves both in the organic and inorganic world. But, besides that some links of the chain which once existed are now clearly lost and others concealed from view, we have good reason to suspect that it was never complete originally. It may undoubtedly be said that strata have been always forming somewhere, and therefore at every moment of past time nature has added a page to her archives; but, in reference to this subject, it should be remembered that we can never hope to compile a consecutive history by gathering together monuments which were originally detached and scattered over the globe. For as the species of organic beings contemporaneously inhabiting remote regions are distinct, the fossils of the first of several periods which may be preserved in any one country, as in America, for example, will have no connection with those of a second period found in India, and will therefore no more enable us to trace the signs of a gradual change in the living creation than a fragment of Chinese history will fill up a blank in the political annals of Europe.

How far some of the great violations of continuity which now exist in the chronological table of fossiliferous rocks will hereafter be removed or lessened must at present be mere matter of conjecture. The hiatus which exists in Great Britain between the fossils of the Lias and those of the Magnesian Limestone is supplied in Germany by the rich fauna and flora of the Muschelkalk, Keuper, and Bunter Sandstein, which we know to be of a date precisely intermediate; those three formations being interposed in Germany between others which agree perfectly in their organic remains with our Lias and Magnesian Limestone. Still we must expect, for reasons before stated, that some such chasms will forever continue to occur in some parts of our sedimentary series.

Consistency of the theory of gradual change, with the existence of great breaks in the series.—To return to the general argument pursued in this chapter, it is assumed, for reasons above explained, that a slow change of species is in simultaneous operation everywhere throughout the habitable surface of sea and land; whereas the fossilization of plants and animals is confined to those areas where new strata are produced. These areas, as we have seen, are always shifting their position; so that the fossilizing process, by means of which the commemoration of the particular

state of the organic world, at any given time, is affected, may be said to move about, visiting and revisiting different tracts in succession.

To make still more clear the supposed working of this machinery, I shall compare it to a somewhat analogous case that might be imagined to occur in the history of human affairs.

Suppose we had discovered two buried cities at the foot of Vesuvius, immediately superimposed upon each other, with a great mass of tuff and lava intervening, just as Portici and Resina, if now covered with ashes, would overlie Herculaneum. An antiquary might possibly be entitled to infer, from the inscriptions on public edifices, that the inhabitants of the inferior and older city were Greeks, and those of the modern towns Italians. But he would reason very hastily if he also concluded from these data that there had been a sudden change from the Greek to the Italian language in Campania. But if he afterwards found *three* buried cities, one above the other, the intermediate one being Roman, while, as in the former example, the lowest was Greek and the uppermost Italian, he would then perceive the fallacy of his former opinion, and would begin to suspect that the catastrophes, by which the cities were inhumed, might have no relation whatever to the fluctuations in the language of the inhabitants: and that, as the Roman tongue had evidently intervened between the Greek and Italian, so many other dialects may have been spoken in succession, and the passage from the Greek to the Italian may have been very gradual; some terms growing obsolete, while others were introduced from time to time.

If this antiquary could have shown that the volcanic paroxysms of Vesuvius were so governed as that cities should be buried one above the other, just as often as any variation occurred in the language of the inhabitants, then, indeed, the abrupt passage from a Greek to a Roman, and from a Roman to an Italian city, would afford proof of fluctuations no less sudden in the language of the people.

So, in Geology, if we could assume that it is part of the plan of Nature to preserve, in every region of the globe, an unbroken series of monuments to commemorate the vicissitudes of the organic creation, we might infer the sudden extirpation of species, and the simultaneous introduction of others, as often as two formations in contact are found to include dissimilar organic fossils. But we must shut our eyes to the whole economy of the existing causes, aqueous, igneous, and organic, if we fail to perceive *that such is not the plan of Nature.*

Concluding remarks on the identity of the ancient and present system of terrestrial changes.—I shall now conclude the discussion of whether there has been any interruption, from the remotest periods, of one uniform system of change in the animate and inanimate world. We were induced to enter into that inquiry by reflecting how much the progress of opinion in Geology had been influenced by the assumption that the analogy was slight in kind, and still more slight in degree, between the causes which produced the former revolutions of the globe and those now in everyday operation.

Never was there a dogma more calculated to foster indolence, and to blunt the keen edge of curiosity, than this assumption of the discordance between the ancient and existing causes of change. It produced a state of mind unfavourable in the highest degree to the candid reception of the evidence of those minute but incessant alterations which every part of the earth's surface is undergoing, and by which the condition of its living inhabitants is continually made to vary. The student, instead of being encouraged with the hope of interpreting the enigmas presented to him in the earth's structure,—instead of being prompted to undertake laborious inquiries into the natural history of the organic world, and the complicated effects of the igneous and aqueous causes now in operation, was taught to despond from the first. Geology, it was affirmed, could never rise to the rank of an exact science—the greater number of phenomena must forever remain inexplicable, or only be partially elucidated by ingenious conjectures. Even the mystery which invested the subject was said to constitute one of its principal charms, affording, as it did, full scope to the fancy to indulge in a boundless field of speculation.

The course directly opposed to this method of philosophizing consists in an earnest and patient inquiry, how far geological appearances are reconcilable with the effect of changes now in progress, or which may be in progress in regions inaccessible to us, and of which the reality is attested by volcanos and subterranean movements. It also endeavours to estimate the aggregate result of ordinary operations multiplied by time, and cherishes a sanguine hope that the resources to be derived from observation and experiment, or from the study of nature such as she now is, are very far from being exhausted. For this reason all theories are rejected which involve the assumption of sudden and violent catastrophes and revolutions of the whole earth, and its inhabitants—theories which are restrained by no reference to existing analogies, and in which a desire is manifested to cut, rather than patiently to untie, the Gordian knot.

We have now, at least, the advantage of knowing, from experience, that an opposite method has always put geologists on the road that leads to truth—suggesting views which, although imperfect at first, have been found capable of improvement, until at last adopted by universal consent; while the method of speculating on a former distinct state of things and causes has led invariably to a multitude of contradictory systems, which have been overthrown one after the other—have been found incapable of modification—and which have often required to be precisely reversed.

THE ORIGIN OF SPECIES

by

CHARLES DARWIN

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The Origin of Species

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CHARLES DARWIN

1809–1882

BORN on February 12, 1809, in Shrewsbury, the fifth child of Dr. Robert Darwin, Charles Darwin was descended on his mother's side from the great ceramic manufacturer, Josiah Wedgwood, and on his father's from Erasmus Darwin, the naturalist and poet. He was a docile, amiable child, much given to daydreaming, and, after his mother's death when he was five, to long, solitary cross-country walks. He had a quite undistinguished residence at a boarding school, then at Edinburgh, where he displayed small interest in the medical lectures he had gone to attend, then at Cambridge, where he showed no more aptitude for theology than he had for medicine. When he left Cambridge in 1831, his friends and his family knew him as a generous, energetic lad who enjoyed shooting, riding, gambling, and gay dinner parties; who had dabbled a bit in chemistry and had barely survived his various scholastic examinations; who had haphazardly collected coins, minerals, and beetles. Once, after hearing Adam Sedgwick lecture on geology, he had made a holiday geological expedition into North Wales. Once, after reading Alexander von Humboldt's *Personal Narrative,* he had become sufficiently enthusiastic for a naturalist's journeys and life to study Spanish in the hope of making a naturalist's expedition to Teneriffe. Of these half-developed tastes, his Cambridge lecturer in botany, John Stevens Henslow, knew. In 1831 he secured for young Darwin an appointment as unsalaried naturalist for the *Beagle,* a 250-ton brig which was about to sail on a long survey of South America and to make chronometrical measurements round the world. On December 27, 1831, the *Beagle* sailed, carrying the unknown Darwin on an expedition which lasted almost five years.

For his work as naturalist, Darwin was assigned as office and shop a space in the chartroom so narrow that he was

forced to develop habits of order. The *Beagle* visited, in turn, the Cape Verde Islands, the coasts of South America, the Galapagos, Tahiti, New Zealand, Australia, Tasmania, Mauritius, Ascension, the Azores. Gradually Darwin made himself a first-rate collector, an observant, shrewd geologist. He puzzled over the fossils of the South American continent, over the birds of Galapagos; he faced the vexed problem of the origin of species.

On the return of the *Beagle* to England in 1836, Darwin busied himself for several years in Cambridge and London with his collections, some geological reports, and with the writing of his *Journal.* In 1839 he married his cousin, Emma Wedgwood, and three years later they went to live in Down, a village fifteen miles from London. Darwin's health had declined almost from the time of the *Beagle's* return; during his remaining forty years he was almost constantly ill. He developed a routine of working very hard for so long as his constitution allowed, then taking a brief holiday rest, then plunging again into work. Except for these brief holiday jaunts and for short trips to such meetings as those of the British Society, he spent these forty years almost wholly in his own house and garden at Down.

Before moving to Down, Darwin had given his fossil collection to the College of Surgeons; with the aid of a Treasury grant he had published the quarto volumes *Zoology of the Voyage of the Beagle;* he had read several papers to the Geological Society and had served three years as the Society's secretary. In the early years at Down (1842–46) he wrote *Volcanic Islands* and *Geology of South America,* propounded a theory—immediately accepted by other geologists—on the origin of coral reefs, and prepared a second edition of his *Journal.* Then he devoted eight years to the preparation for the Paleontological Society and the Royal Society (1851, 1854) of his definitive monograph on the cirripides. During this long labor he learned taxonomy and morphology, thus completing the education of a naturalist begun on the *Beagle.*

Though Darwin had pondered the problem of the origin of species while he was aboard the *Beagle,* it was the reading of Malthus's theories on population which crystallized his own ideas. In 1842 he set these ideas down in a sketch thirty-five pages long (unpublished) and two years later expanded the sketch into an essay of 220 pages. Convinced that his theory was revolutionary, he now settled to an arduous course to prepare himself for writing a developed book on the subject. For fourteen years he read books of travel, books on natural history, horticulture, animal husbandry; he read whole files of journals. Always he took copious notes, constantly or-

ganizing and filing them. He prepared skeletons of domesticated birds in order to compare their bones with those of wild species. He kept pigeons and did experiments in crossbreeding. He corresponded voluminously with Charles Lyell, Asa Gray, and William Jackson Hooker about disputed points of geology, about the geographical distribution of species, about the transportation of seeds. Finally, in 1856, Lyell persuaded him that he had undertaken an endless task of study and that he should publish a book on his findings and theorizings so far. Two years later, when he had just completed ten chapters of the projected book, he received from Alfred Russel Wallace, then in Malaysia, an essay for his criticism. It expressed in detail Darwin's own theory.

Darwin's first inclination was to publish Wallace's paper at once, withdrawing all claim to priority in conceiving his theory of the origin of species. Lyell and Hooker counseled differently. On their advice, Wallace's essay and some portions of Darwin's 1844 essay were presented to the Linnaean Society for publication in its *Journal* in 1858. Immediately abandoning his great book, Darwin began preparing an "abstract" of it. This was ready in a year and appeared in 1859: *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life.*

Partly because of the great exciting phrases in its title, partly because the topic of evolution—advanced earlier by Lamarck and temperately discussed by Lyell—was already in the air, the first edition (1,200 copies) of this book sold out on the day of publication. Two months later a second edition (3,000 copies) sold almost as rapidly. Its early chapters (I–IV) explain the operations of artificial selection by man, and of natural selection occasioned by the struggle for existence. Then it presents (Chapter V) the laws of variation and causes of modification other than natural selection, exposes fully (Chapters VI–X) the difficulties in believing in evolution and natural selection, and closes with three masterly chapters (XI–XIII) marshaling the evidence for evolution. The theory of natural selection is primarily an explanation of the phenomenon of adaptation. But the explanation makes easy the acceptance of the theory of evolution. Further, it provides a mechanical explanation for what had hitherto required the special-creation hypothesis. Pedantic religious men took the theory to be a denial of God as the creator. Bishop Wilberforce in particular fulminated against Darwin. Huxley came to the defense of the new theory, and a furious contention developed, culminating in the famous Oxford debate of 1860.

Darwin was slow in controversy. He left the defense of his position largely to his polemical friends; but in a scholar's

way he busied himself in consolidating it. In 1868 he published the work of eight years, *The Variation of Animals and Plants Under Domestication,* which is really an elaboration of a part of the *Origin.* In 1871 he published *Descent of Man,* really a continuation of the *Variation.* Much more shocking from an orthodox religious point of view than the *Origin,* the *Descent* roused little bitter hostility. In the preceding twelve years the scientific and intellectual world had almost universally accepted the Darwinian theses.

In the tremendous success he attained, Darwin remained unostentatious. Convinced of the importance of his interpretation of accumulated data, he was sometimes unjust to his predecessors. But he never wearied of observing and collecting facts, of reflecting over any subject patiently in order the better to theorize, of abandoning hypotheses which experiment, common sense, and observation proved untenable, and of clinging pertinaciously to doctrines no matter how radical when experiment, common sense, and observation insured their value.

Darwin's remarkable studies, *On the British Orchids* (1862), *On Climbing Plants* (1864), *Expression of the Emotions in Man and Animals* (1872)—all the results of his omnivorous reading and his enthusiastic experimentation—contributed to the acceptance of his theory. They did more. They revolutionized the work of the natural historian—who thenceforth busied himself retracing zoological history, and of the embryologist—who became a reader of phylogeny in ontogeny, and of the comparative anatomist—who concentrated now on the effect of function and environment in molding bodily form. It is hardly possible, furthermore, to say how much the optimistic nineteenth-century doctrine of progress owed to the theory of organic evolution. The greatest single contribution of the nineteenth century to the world's intellectual history is summed up in the term Darwinism.

THE ORIGIN OF SPECIES

INTRODUCTION

WHEN on board H.M.S. *Beagle,* as naturalist, I was much struck with certain facts in the distribution of the organic beings inhabiting South America, and in the geological relations of the present to the past inhabitants of that continent. These facts, as will be seen in the latter chapters of this volume, seemed to throw some light on the origin of species—that mystery of mysteries, as it has been called by one of our greatest philosophers. On my return home, it occurred to me, in 1837, that something might perhaps be made out on this question by patiently accumulating and reflecting on all sorts of facts which could possibly have any bearing on it. After five years' work I allowed myself to speculate on the subject, and drew up some short notes; these I enlarged in 1844 into a sketch of the conclusions, which then seemed to me probable: from that period to the present day I have steadily pursued the same object. I hope that I may be excused for entering on these personal details, as I give them to show that I have not been hasty in coming to a decision.

This Abstract, which I now publish, must necessarily be imperfect. I cannot here give references and authorities for my several statements; and I must trust to the reader reposing some confidence in my accuracy. No doubt errors will have crept in, though I hope I have always been cautious in trusting to good authorities alone. I can here give only the general conclusions at which I have arrived, with a few facts in illustration, but which, I hope, in most cases will suffice. No one can feel more sensible than I do of the necessity of hereafter publishing in detail all the facts, with references, on which my conclusions have been grounded; and I hope in a future work to do this. For I am well aware that scarcely a single point is discussed in this volume on which facts cannot be adduced, often apparently leading to conclusions directly opposite to those at which I have arrived. A fair result can be obtained only by fully stating and balancing the facts and arguments on both sides of each question; and this is here impossible.

No one ought to feel surprise at much remaining as yet unexplained in regard to the origin of species and varieties, if he make due allowance for our profound ignorance in regard to the mutual relations of the many beings which live around us. Who can explain why one species ranges

widely and is very numerous, and why another allied species has a narrow range and is rare? Yet these relations are of the highest importance, for they determine the present welfare and, as I believe, the future success and modification of every inhabitant of this world. Still less do we know of the mutual relations of the innumerable inhabitants of the world during the many past geological epochs in its history. Although much remains obscure, and will long remain obscure, I can entertain no doubt, after the most deliberate study and dispassionate judgment of which I am capable, that the view which most naturalists until recently entertained, and which I formerly entertained—namely, that each species has been independently created—is erroneous. I am fully convinced that species are not immutable; but that those belonging to what are called the same genera are lineal descendants of some other and generally extinct species, in the same manner as the acknowledged varieties of any one species are the descendants of that species. Furthermore, I am convinced that Natural Selection has been the most important, but not the exclusive, means of modification.

I. VARIATION UNDER DOMESTICATION

Causes of Variability

WHEN we compare the individuals of the same variety or sub-variety of our older cultivated plants and animals, one of the first points which strikes us is that they generally differ more from each other than do the individuals of any one species or variety in a state of nature. And if we reflect on the vast diversity of the plants and animals which have been cultivated, and which have varied during all ages under the most different climates and treatment, we are driven to conclude that this great variability is due to our domestic productions having been raised under conditions of life not so uniform as, and somewhat different from, those to which the parent species had been exposed under nature.

As far as I am able to judge, after long attending to the subject, the conditions of life appear to act in two ways—directly on the whole organisation or on certain parts alone, and indirectly by affecting the reproductive system. With respect to the direct action, we must bear in mind that in every case there are two factors: namely, the nature of the organism, and the nature of the conditions. The former seems to be much the more important; for nearly similar variations sometimes arise under, as far as we can judge, dissimilar conditions; and, on the other hand, dissimilar variations arise under conditions which appear to be nearly uniform. The effects on the offspring are either definite or indefinite. They may be considered as definite when all or nearly all the offspring of individuals exposed to certain conditions during several generations are modified in the same manner.

Indefinite variability is a much more common result of changed con-

ditions than definite variability, and has probably played a more important part in the formation of our domestic races. We see indefinite variability in the endless slight peculiarities which distinguish the individuals of the same species, and which cannot be accounted for by inheritance from either parent or from some more remote ancestor. Even strongly marked differences occasionally appear in the young of the same litter and in seedlings from the same seed capsule. All such changes of structure, whether extremely slight or strongly marked, which appear amongst many individuals living together, may be considered as the indefinite effects of the conditions of life on each individual organism, in nearly the same manner as the chill affects different men in an indefinite manner, according to their state of body or constitution, causing coughs or colds, rheumatism, or inflammation of various organs.

With respect to what I have called the indirect action of changed conditions, namely, through the reproductive system being affected, we may infer that variability is thus induced, partly from the fact of this system being extremely sensitive to any change in the conditions, and partly from the similarity, as Kölreuter and others have remarked, between the variability which follows from the crossing of distinct species and that which may be observed with plants and animals when reared under new or unnatural conditions. Many facts clearly show how eminently susceptible the reproductive system is to very slight changes in the surrounding conditions. Nothing is more easy than to tame an animal, and few things more difficult than to get it to breed freely under confinement, even when the male and female unite. Carnivorous animals, even from the tropics, breed in this country pretty freely under confinement, with the exception of the plantigrades or bear family, which seldom produce young; whereas carnivorous birds, with the rarest exceptions, hardly ever lay fertile eggs. Many exotic plants have pollen utterly worthless, in the same condition as in the most sterile hybrids. When, on the one hand, we see domesticated animals and plants, though often weak and sickly, breeding freely under confinement; and when, on the other hand, we see individuals, though taken young from a state of nature, perfectly tamed, long-lived and healthy (of which I could give numerous instances), yet having their reproductive system so seriously affected by unperceived causes as to fail to act, we need not be surprised at this system, when it does act under confinement, acting irregularly, and producing offspring somewhat unlike their parents.

Effects of Habit and of the Use or Disuse of Parts; Correlated Variation; Inheritance

Changed habits produce an inherited effect, as in the period of the flowering of plants when transported from one climate to another. With animals the increased use or disuse of parts has had a more marked influence; thus I find in the domestic duck that the bones of the wing

weigh less and the bones of the leg more, in proportion to the whole skeleton, than do the same bones in the wild duck; and this change may be safely attributed to the domestic duck flying much less, and walking more, than its wild parents. Not one of our domestic animals can be named which has not in some country drooping ears; and the view which has been suggested, that the drooping is due to disuse of the muscles of the ear, from the animals being seldom much alarmed, seems probable.

Many laws regulate variation, some few of which can be dimly seen, and will hereafter be briefly discussed. I will here only allude to what may be called correlated variation. Breeders believe that long limbs are almost always accompanied by an elongated head. Some instances of correlation are quite whimsical: thus cats which are entirely white and have blue eyes are generally deaf; but it has been lately stated by Mr. Tait that this is confined to the males. Colour and constitutional peculiarities go together, of which many remarkable cases could be given amongst animals and plants. From facts collected by Heusinger, it appears that white sheep and pigs are injured by certain plants, whilst dark-coloured individuals escape: Professor Wyman has recently communicated to me a good illustration of this fact; on asking some farmers in Virginia how it was that all their pigs were black, they informed him that the pigs ate the paint-root (Lachnanthes), which coloured their bones pink, and which caused the hoofs of all but the black varieties to drop off; and one of the "crackers" (*i.e.* Virginia squatters) added, "we select the black members of a litter for raising, as they alone have a good chance of living." Hairless dogs have imperfect teeth; long-haired and coarse-haired animals are apt to have, as is asserted, long or many horns; pigeons with feathered feet have skin between their outer toes; pigeons with short beaks have small feet, and those with long beaks large feet. Hence if man goes on selecting, and thus augmenting, any peculiarity, he will almost certainly modify unintentionally other parts of the structure, owing to the mysterious laws of correlation.

Any variation which is not inherited is unimportant for us. But the number and diversity of inheritable deviations of structure, both those of slight and those of considerable physiological importance, are endless. No breeder doubts how strong is the tendency to inheritance; that like produces like is his fundamental belief: doubts have been thrown on this principle only by theoretical writers. When any deviation of structure often appears, and we see it in the father and child, we cannot tell whether it may not be due to the same cause having acted on both; but when amongst individuals, apparently exposed to the same conditions, any very rare deviation, due to some extraordinary combination of circumstances, appears in the parent—say, once amongst several million individuals—and it reappears in the child, the mere doctrine of chances almost compels us to attribute its reappearance to inheritance. Everyone must have heard of cases of albinism, prickly skin, hairy bodies, &c., appearing in several members of the same family. If strange and rare deviations of structure are really inherited, less strange and commoner deviations may be freely

admitted to be inheritable. Perhaps the correct way of viewing the whole subject would be to look at the inheritance of every character whatever as the rule, and non-inheritance as the anomaly.

Character of Domestic Varieties; difficulty of distinguishing between Varieties and Species; origin of Domestic Varieties from one or more Species

When we look to the hereditary varieties or races of our domestic animals and plants, and compare them with closely allied species, we generally perceive in each domestic race, as already remarked, less uniformity of character than in true species.

It has often been assumed that man has chosen for domestication animals and plants having an extraordinary inherent tendency to vary, and likewise to withstand diverse climates. I do not dispute that these capacities have added largely to the value of most of our domesticated productions: but how could a savage possibly know, when he first tamed an animal, whether it would vary in succeeding generations, and whether it would endure other climates? Has the little variability of the ass and goose, or the small power of endurance of warmth by the reindeer, or of cold by the common camel, prevented their domestication? I cannot doubt that if other animals and plants, equal in number to our domesticated productions, and belonging to equally diverse classes and countries, were taken from a state of nature, and could be made to breed for an equal number of generations under domestication, they would on an average vary as largely as the parent species of our existing domesticated productions have varied.

The doctrine of the origin of our several domestic races from several aboriginal stocks has been carried to an absurd extreme by some authors. They believe that every race which breeds true, let the distinctive characters be ever so slight, has had its wild prototype. At this rate there must have existed at least a score of species of wild cattle, as many sheep, and several goats, in Europe alone, and several even within Great Britain. Even in the case of the breeds of the domestic dog throughout the world, which I admit are descended from several wild species, it cannot be doubted that there has been an immense amount of inherited variation; for who will believe that animals closely resembling the Italian greyhound, the bloodhound, the bulldog, pug dog, or Blenheim spaniel, &c.—so unlike all wild Canidæ—ever existed in a state of nature? It has often been loosely said that all our races of dogs have been produced by the crossing of a few aboriginal species; but by crossing we can only get forms in some degree intermediate between their parents; and if we account for our several domestic races by this process, we must admit the former existence of the most extreme forms, as the Italian greyhound, bloodhound, bulldog, &c., in the wild state. Moreover, the possibility of making distinct races by crossing has been greatly exaggerated. Many cases are on

record, showing that a race may be modified by occasional crosses, if aided by the careful selection of the individuals which present the desired character; but to obtain a race intermediate between two quite distinct races would be very difficult. Sir J. Sebright expressly experimented with this object and failed. The offspring from the first cross between two pure breeds is tolerably and sometimes (as I have found with pigeons) quite uniform in character, and everything seems simple enough; but when these mongrels are crossed one with another for several generations, hardly two of them are alike, and then the difficulty of the task becomes manifest.

Breeds of the Domestic Pigeon, their Differences and Origin

Believing that it is always best to study some special group, I have, after deliberation, taken up domestic pigeons. The diversity of the breeds is something astonishing. Compare the English carrier and the short-faced tumbler, and see the wonderful difference in their beaks, entailing corresponding differences in their skulls. The carrier, more especially the male bird, is also remarkable from the wonderful development of the carunculated skin about the head; and this is accompanied by greatly elongated eyelids, very large external orifices to the nostrils, and a wide gape of mouth. The short-faced tumbler has a beak in outline almost like that of a finch; and the common tumbler has the singular inherited habit of flying at a great height in a compact flock, and tumbling in the air head over heels. The runt is a bird of great size, with long massive beak and large feet; some of the sub-breeds of runts have very long necks, others very long wings and tails, others singularly short tails. The barb is allied to the carrier, but, instead of a long beak, has a very short and broad one. The pouter has a much elongated body, wings, and legs; and its enormously developed crop, which it glories in inflating, may well excite astonishment and even laughter. The turbit has a short and conical beak, with a line of reversed feathers down the breast; and it has the habit of continually expanding slightly the upper part of the œsophagus. The Jacobin has the feathers so much reversed along the back of the neck that they form a hood; and it has, proportionally to its size, elongated wing and tail feathers. The trumpeter and laugher, as their names express, utter a very different coo from the other breeds. The fantail has thirty or even forty tail feathers, instead of twelve or fourteen—the normal number in all the members of the great pigeon family: these feathers are kept expanded, and are carried so erect that in good birds the head and tail touch: the oil gland is quite aborted.

Altogether at least a score of pigeons might be chosen, which, if shown to an ornithologist, and he were told that they were wild birds, would certainly be ranked by him as well-defined species. Moreover, I do not believe that any ornithologist would in this case place the English carrier, the short-faced tumbler, the runt, the barb, pouter, and fantail in

the same genus; more especially as in each of these breeds several truly inherited sub-breeds, or species, as he would call them, could be shown him.

Great as are the differences between the breeds of the pigeon, I am fully convinced that the common opinion of naturalists is correct, namely, that all are descended from the rock pigeon (Columba livia), including under this term several geographical races or sub-species, which differ from each other in the most trifling respects. As several of the reasons which have led me to this belief are in some degree applicable in other cases, I will here briefly give them. If the several breeds are not varieties, and have not proceeded from the rock pigeon, they must have descended from at least seven or eight aboriginal stocks; for it is impossible to make the present domestic breeds by the crossing of any lesser number: how, for instance, could a pouter be produced by crossing two breeds unless one of the parent stocks possessed the characteristic enormous crop? The supposed aboriginal stocks must all have been rock pigeons, that is, they did not breed or willingly perch on trees. But besides C. livia, with its geographical sub-species, only two or three other species of rock pigeons are known; and these have not any of the characters of the domestic breeds. Hence the supposed aboriginal stocks must either still exist in the countries where they were originally domesticated, and yet be unknown to ornithologists; and this, considering their size, habits, and remarkable characters, seems improbable; or they must have become extinct in the wild state. But birds breeding on precipices, and good fliers, are unlikely to be exterminated; and the common rock pigeon, which has the same habits with the domestic breeds, has not been exterminated even on several of the smaller British islets, or on the shores of the Mediterranean. Hence the supposed extermination of so many species having similar habits with the rock pigeon seems a very rash assumption. Moreover, the several above-named domesticated breeds have been transported to all parts of the world, and, therefore, some of them must have been carried back again into their native country; but not one has become wild or feral, though the dovecot pigeon, which is the rock pigeon in a very slightly altered state, has become feral in several places.

Some facts in regard to the colouring of pigeons well deserve consideration. The rock pigeon is of a slaty-blue, with white loins; but the Indian sub-species, C. intermedia of Strickland, has this part bluish. The tail has a terminal dark bar, with the outer feathers externally edged at the base with white. The wings have two black bars. Some semi-domestic breeds, and some truly wild breeds, have, besides the two black bars, the wings chequered with black. These several marks do not occur together in any other species of the whole family. Now, in every one of the domestic breeds, taking thoroughly well-bred birds, all the above marks, even to the white edging of the outer tail feathers, sometimes concur perfectly developed. Moreover, when birds belonging to two or more distinct breeds are crossed, none of which are blue or have any of the above-specified marks, the mongrel offspring are very apt suddenly to acquire these

characters. To give one instance out of several which I have observed:—I crossed some white fantails, which breed very true, with some black barbs —and it so happens that blue varieties of barbs are so rare that I never heard of an instance in England; and the mongrels were black, brown, and mottled. I also crossed a barb with a spot, which is a white bird with a red tail and red spot on the forehead, and which notoriously breeds very true; the mongrels were dusky and mottled. I then crossed one of the mongrel barb fantails with a mongrel barb spot, and they produced a bird of as beautiful a blue colour, with the white loins, double black wing bar, and barred and white-edged tail feathers, as any wild rock pigeon! We can understand these facts, on the well-known principle of reversion to ancestral characters, if all the domestic breeds are descended from the rock pigeon. But if we deny this, we must make one of the two following highly improbable suppositions. Either, first, that all the several imagined aboriginal stocks were coloured and marked like the rock pigeon, although no other existing species is thus coloured and marked, so that in each separate breed there might be a tendency to revert to the very same colours and markings. Or, secondly, that each breed, even the purest, has within a dozen, or at most within a score, of generations been crossed by the rock pigeon: I say within a dozen or twenty generations, for no instance is known of crossed descendants reverting to an ancestor of foreign blood, removed by a greater number of generations. In a breed which has been crossed only once, the tendency to revert to any character derived from such a cross will naturally become less and less, as in each succeeding generation there will be less of the foreign blood; but when there has been no cross, and there is a tendency in the breed to revert to a character which was lost during some former generation, this tendency, for all that we can see to the contrary, may be transmitted undiminished for an indefinite number of generations. These two distinct cases of reversion are often confounded together by those who have written on inheritance.

From these several reasons, namely—the improbability of man having formerly made seven or eight supposed species of pigeons to breed freely under domestication;—these supposed species being quite unknown in a wild state, and their not having become anywhere feral;—these species presenting certain very abnormal characters, as compared with all other Columbidæ, though so like the rock pigeon in most respects;—the occasional reappearance of the blue colour and various black marks in all the breeds, both when kept pure and when crossed;—and lastly, the mongrel offspring being perfectly fertile;—from these several reasons taken together, we may safely conclude that all our domestic breeds are descended from the rock pigeon or Columba livia with its geographical sub-species.

In favour of this view, I may add, firstly, that the wild C. livia has been found capable of domestication in Europe and in India; and that it agrees in habits and in a great number of points of structure with all the domestic breeds. Secondly, that, although an English carrier or a short-faced tumbler differs immensely in certain characters from the rock

pigeon, yet that, by comparing the several sub-breeds of these two races, more especially those brought from distant countries, we can make, between them and the rock pigeon, an almost perfect series; so we can in some other cases, but not with all the breeds. Thirdly, those characters which are mainly distinctive of each breed are in each eminently variable, for instance, the wattle and length of beak of the carrier, the shortness of that of the tumbler, and the number of tail feathers in the fantail; and the explanation of this fact will be obvious when we treat of Selection. Fourthly, pigeons have been watched and tended with the utmost care, and loved by many people. They have been domesticated for thousands of years in several quarters of the world; the earliest known record of pigeons is in the fifth Ægyptian dynasty, about 3000 B.C., as was pointed out to me by Professor Lepsius; but Mr. Birch informs me that pigeons are given in a bill of fare in the previous dynasty. In the time of the Romans, as we hear from Pliny, immense prices were given for pigeons; "nay, they are come to this pass, that they can reckon up their pedigree and race." Pigeons were much valued by Akber Khan in India, about the year 1600; never less than 20,000 pigeons were taken with the court. "The monarchs of Iran and Turan sent him some very rare birds," and, continues the courtly historian, "His Majesty, by crossing the breeds, which method was never practised before, has improved them astonishingly." About this same period the Dutch were as eager about pigeons as were the old Romans. The paramount importance of these considerations, in explaining the immense amount of variation which pigeons have undergone, will likewise be obvious when we treat of Selection. We shall then, also, see how it is that the several breeds so often have a somewhat monstrous character.

I have discussed the probable origin of domestic pigeons at some, yet quite insufficient, length; because when I first kept pigeons and watched the several kinds, well knowing how truly they breed, I felt fully as much difficulty in believing that since they had been domesticated they had all proceeded from a common parent, as any naturalist could in coming to a similar conclusion in regard to the many species of finches, or other groups of birds, in nature. One circumstance has struck me much; namely, that nearly all the breeders of the various domestic animals and the cultivators of plants, with whom I have conversed, or whose treatises I have read, are firmly convinced that the several breeds to which each has attended are descended from so many aboriginally distinct species. Ask, as I have asked, a celebrated raiser of Hereford cattle whether his cattle might not have descended from Longhorns, or both from a common parent stock, and he will laugh you to scorn. I have never met a pigeon, or poultry, or duck, or rabbit fancier who was not fully convinced that each main breed was descended from a distinct species. Van Mons, in his treatise on pears and apples, shows how utterly he disbelieves that the several sorts, for instance, a Ribston pippin or Codlin apple, could ever have proceeded from the seeds of the same tree. Innumerable other examples could be given. The explanation, I think, is simple: from long-

continued study they are strongly impressed with the differences between the several races; and though they well know that each race varies slightly, for they win their prizes by selecting such slight differences, yet they ignore all general arguments, and refuse to sum up in their minds slight differences accumulated during many successive generations. May not those naturalists who, knowing far less of the laws of inheritance than does the breeder, and knowing no more than he does of the intermediate links in the long lines of descent, yet admit that many of our domestic races are descended from the same parents—may they not learn a lesson of caution, when they deride the idea of species in a state of nature being lineal descendants of other species?

Principles of Selection anciently followed, and their Effects

Let us now briefly consider the steps by which domestic races have been produced, either from one or from several allied species. Some effect may be attributed to the direct and definite action of the external conditions of life, and some to habit; but he would be a bold man who would account by such agencies for the differences between a dray and race horse, a greyhound and bloodhound, a carrier and tumbler pigeon. One of the most remarkable features in our domesticated races is that we see in them adaptation, not indeed to the animal's or plant's own good, but to man's use or fancy. Some variations useful to him have probably arisen suddenly, or by one step; many botanists, for instance, believe that the fuller's teasel, with its hooks, which cannot be rivalled by any mechanical contrivance, is only a variety of the wild Dipsacus; and this amount of change may have suddenly arisen in a seedling. So it has probably been with the turnspit dog; and this is known to have been the case with the ancon sheep. But when we compare the dray horse and race horse, the dromedary and camel, the various breeds of sheep fitted either for cultivated land or mountain pasture, with the wool of one breed good for one purpose, and that of another breed for another purpose; when we compare the many breeds of dogs, each good for man in different ways; when we compare the gamecock, so pertinacious in battle, with other breeds so little quarrelsome, with "everlasting layers" which never desire to sit, and with the bantam so small and elegant; when we compare the host of agricultural, culinary, orchard, and flower-garden races of plants, most useful to man at different seasons and for different purposes, or so beautiful in his eyes, we must, I think, look further than to mere variability. We cannot suppose that all the breeds were suddenly produced as perfect and as useful as we now see them; indeed, in many cases, we know that this has not been their history. The key is man's power of accumulative selection: nature gives successive variations; man adds them up in certain directions useful to him. In this sense he may be said to have made for himself useful breeds.

The great power of this principle of selection is not hypothetical.

What English breeders have actually effected is proved by the enormous prices given for animals with a good pedigree; and these have been exported to almost every quarter of the world. The improvement is by no means generally due to crossing different breeds; all the best breeders are strongly opposed to this practice, except sometimes amongst closely allied sub-breeds. And when a cross has been made, the closest selection is far more indispensable even than in ordinary cases. If selection consisted merely in separating some very distinct variety, and breeding from it, the principle would be so obvious as hardly to be worth notice; but its importance consists in the great effect produced by the accumulation in one direction, during successive generations, of differences absolutely inappreciable by an uneducated eye—differences which I for one have vainly attempted to appreciate. Not one man in a thousand has accuracy of eye and judgment sufficient to become an eminent breeder. If gifted with these qualities, and he studies his subject for years, and devotes his lifetime to it with indomitable perseverance, he will succeed, and may make great improvements; if he wants any of these qualities, he will assuredly fail. Few would readily believe in the natural capacity and years of practice requisite to become even a skilful pigeon fancier.

It may be objected that the principle of selection has been reduced to methodical practice for scarcely more than three quarters of a century; it has certainly been more attended to of late years, and many treatises have been published on the subject; and the result has been, in a corresponding degree, rapid and important. But it is very far from true that the principle is a modern discovery. The principle of selection I find distinctly given in an ancient Chinese encyclopædia. Explicit rules are laid down by some of the Roman classical writers. From passages in Genesis, it is clear that the colour of domestic animals was at that early period attended to. Savages now sometimes cross their dogs with wild canine animals, to improve the breed, and they formerly did so, as is attested by passages in Pliny. The savages in South Africa match their draught cattle by colour, as do some of the Esquimaux their teams of dogs. Livingstone states that good domestic breeds are highly valued by the Negroes in the interior of Africa who have not associated with Europeans. Some of these facts do not show actual selection, but they show that the breeding of domestic animals was carefully attended to in ancient times, and is now attended to by the lowest savages. It would, indeed, have been a strange fact had attention not been paid to breeding, for the inheritance of good and bad qualities is so obvious.

Unconscious Selection

At the present time, eminent breeders try by methodical selection, with a distinct object in view, to make a new strain or sub-breed, superior to anything of the kind in the country. But, for our purpose, a form of Selection, which may be called Unconscious, and which results from every-

one trying to possess and breed from the best individual animals, is more important. Thus, a man who intends keeping pointers naturally tries to get as good dogs as he can, and afterwards breeds from his own best dogs, but he has no wish or expectation of permanently altering the breed. Nevertheless we may infer that this process, continued during centuries, would improve and modify any breed. Some highly competent authorities are convinced that the setter is directly derived from the spaniel, and has probably been slowly altered from it. It is known that the English pointer has been greatly changed within the last century, and in this case the change has, it is believed, been chiefly effected by crosses with the fox-hound; but what concerns us is that the change has been effected unconsciously and gradually, and yet so effectually that, though the old Spanish pointer certainly came from Spain, Mr. Borrow has not seen, as I am informed by him, any native dog in Spain like our pointer.

On the view here given of the important part which selection by man has played, it becomes at once obvious how it is that our domestic races show adaptation in their structure or in their habits to man's wants or fancies. We can, I think, further understand the frequently abnormal characters of our domestic races, and likewise their differences being so great in external characters and relatively so slight in internal parts or organs. Man can hardly select, or only with much difficulty, any deviation of structure excepting such as is externally visible; and indeed he rarely cares for what is internal. He can never act by selection, excepting on variations which are first given to him in some slight degree by nature. No man would ever try to make a fantail till he saw a pigeon with a tail developed in some slight degree in an unusual manner, or a pouter till he saw a pigeon with a crop of somewhat unusual size; and the more abnormal or unusual any character was when it first appeared, the more likely it would be to catch his attention. But to use such an expression as trying to make a fantail is, I have no doubt, in most cases utterly incorrect. The man who first selected a pigeon with a slightly larger tail never dreamed what the descendants of that pigeon would become through long-continued, partly unconscious and partly methodical, selection. Perhaps the parent bird of all fantails had only fourteen tail feathers somewhat expanded, like the present Java fantail, or like individuals of other and distinct breeds, in which as many as seventeen tail feathers have been counted. Perhaps the first pouter pigeon did not inflate its crop much more than the turbit now does the upper part of its œsophagus—a habit which is disregarded by all fanciers, as it is not one of the points of the breed.

Nor let it be thought that some great deviation of structure would be necessary to catch the fancier's eye: he perceives extremely small differences, and it is in human nature to value any novelty, however slight, in one's own possession. Nor must the value which would formerly have been set on any slight differences in the individuals of the same species be judged of by the value which is now set on them, after several breeds have fairly been established. It is known that with pigeons many slight varia-

tions now occasionally appear, but these are rejected as faults or deviations from the standard of perfection in each breed. The common goose has not given rise to any marked varieties; hence the Toulouse and the common breed, which differ only in colour, that most fleeting of characters, have lately been exhibited as distinct at our poultry shows.

These views appear to explain what has sometimes been noticed—namely, that we know hardly anything about the origin or history of any of our domestic breeds. But, in fact, a breed, like a dialect of a language, can hardly be said to have a distinct origin. A man preserves and breeds from an individual with some slight deviation of structure, or takes more care than usual in matching his best animals, and thus improves them, and the improved animals slowly spread in the immediate neighbourhood. But they will as yet hardly have a distinct name, and from being only slightly valued, their history will have been disregarded. When further improved by the same slow and gradual process, they will spread more widely, and will be recognised as something distinct and valuable, and will then probably first receive a provincial name. In semi-civilised countries, with little free communication, the spreading of a new sub-breed would be a slow process. As soon as the points of value are once acknowledged, the principle, as I have called it, of unconscious selection will always tend—perhaps more at one period than at another, as the breed rises or falls in fashion—perhaps more in one district than in another, according to the state of civilisation of the inhabitants—slowly to add to the characteristic features of the breed, whatever they may be. But the chance will be infinitely small of any record having been preserved of such slow, varying, and insensible changes.

Circumstances favourable to Man's Power of Selection

I will now say a few words on the circumstances favourable, or the reverse, to man's power of selection. A high degree of variability is obviously favourable, as freely giving the materials for selection to work on; not that mere individual differences are not amply sufficient, with extreme care, to allow of the accumulation of a large amount of modification in almost any desired direction. But as variations manifestly useful or pleasing to man appear only occasionally, the chance of their appearance will be much increased by a large number of individuals being kept. Hence, number is of the highest importance for success. Nurserymen, from keeping large stocks of the same plant, are generally far more successful than amateurs in raising new and valuable varieties. A large number of individuals of an animal or plant can be reared only where the conditions for its propagation are favourable. When the individuals are scanty, all will be allowed to breed, whatever their quality may be, and this will effectually prevent selection. But probably the most important element is that the animal or plant should be so highly valued by man that the closest attention is paid to even the slightest deviations in its qualities or struc-

ture. Unless such attention be paid nothing can be effected. I have seen it gravely remarked that it was most fortunate that the strawberry began to vary just when gardeners began to attend to this plant. No doubt the strawberry had always varied since it was cultivated, but the slightest varieties had been neglected. As soon, however, as gardeners picked out individual plants with slightly larger, earlier, or better fruit, and raised seedlings from them, and again picked out the best seedlings and bred from them, then (with some aid by crossing distinct species) those many admirable varieties of the strawberry were raised which have appeared during the last half century.

With animals, facility in preventing crosses is an important element in the formation of new races—at least, in a country which is already stocked with other races. In this respect enclosure of the land plays a part. Wandering savages or the inhabitants of open plains rarely possess more than one breed of the same species. Pigeons can be mated for life, and this is a great convenience to the fancier, for thus many races may be improved and kept true, though mingled in the same aviary; and this circumstance must have largely favoured the formation of new breeds. Pigeons, I may add, can be propagated in great numbers and at a very quick rate, and inferior birds may be freely rejected, as when killed they serve for food. On the other hand, cats, from their nocturnal rambling habits, cannot be easily matched, and, although so much valued by women and children, we rarely see a distinct breed long kept up; such breeds as we do sometimes see are almost always imported from some other country. Although I do not doubt that some domestic animals vary less than others, yet the rarity or absence of distinct breeds of the cat, the donkey, peacock, goose, &c., may be attributed in main part to selection not having been brought into play: in cats, from the difficulty in pairing them; in donkeys, from only a few being kept by poor people, and little attention paid to their breeding; for recently in certain parts of Spain and of the United States this animal has been surprisingly modified and improved by careful selection.

To sum up on the origin of our domestic races of animals and plants. Changed conditions of life are of the highest importance in causing variability, both by acting directly on the organisation and indirectly by affecting the reproductive system. It is not probable that variability is an inherent and necessary contingent, under all circumstances. The greater or less force of inheritance and reversion determine whether variations shall endure. Variability is governed by many unknown laws, of which correlated growth is probably the most important. Something, but how much we do not know, may be attributed to the definite action of the conditions of life. Some, perhaps a great, effect may be attributed to the increased use or disuse of parts. Over all these causes of Change, the accumulative action of Selection, whether applied methodically and quickly or unconsciously and slowly but more efficiently, seems to have been the predominant Power.

II. VARIATION UNDER NATURE

BEFORE applying the principles arrived at in the last chapter to organic beings in the state of nature, we must briefly discuss whether these latter are subject to any variation. To treat this subject properly, a long catalogue of dry facts ought to be given; but these I shall reserve for a future work. Nor shall I here discuss the various definitions which have been given of the term species. No one definition has satisfied all naturalists; yet every naturalist knows vaguely what he means when he speaks of a species. Generally the term includes the unknown element of a distant act of creation. The term "variety" is almost equally difficult to define; but here community of descent is almost universally implied, though it can rarely be proved.

Individuals of the same species often present, as is known to everyone, great differences of structure, independently of variation, as in the two sexes of various animals, in the two or three castes of sterile females or workers amongst insects, and in the immature and larval states of many of the lower animals. There are, also, cases of dimorphism and trimorphism, both with animals and plants. Thus, Mr. Wallace, who has lately called attention to the subject, has shown that the females of certain species of butterflies, in the Malayan archipelago, regularly appear under two or even three conspicuously distinct forms, not connected by intermediate varieties.

The many slight differences which appear in the offspring from the same parents, or which it may be presumed have thus arisen, from being observed in the individuals of the same species inhabiting the same confined locality, may be called individual differences. No one supposes that all the individuals of the same species are cast in the same actual mould. These individual differences are of the highest importance for us, for they are often inherited, as must be familiar to everyone; and they thus afford materials for natural selection to act on and accumulate, in the same manner as man accumulates in any given direction individual differences in his domesticated productions. These individual differences generally affect what naturalists consider unimportant parts; but I could show by a long catalogue of facts that parts which must be called important, whether viewed under a physiological or classificatory point of view, sometimes vary in the individuals of the same species. It certainly at first appears a highly remarkable fact that the same female butterfly should have the power of producing at the same time three distinct female forms and a male; and that an hermaphrodite plant should produce from the same seed capsule three distinct hermaphrodite forms, bearing three different kinds of females and three or even six different kinds of males. Nevertheless these cases are only exaggerations of the common fact that the female produces offspring of two sexes which sometimes differ from each other in a wonderful manner.

The forms which possess in some considerable degree the character of species, but which are so closely similar to other forms, or are so closely linked to them by intermediate gradations, that naturalists do not like to rank them as distinct species, are in several respects the most important for us. We have every reason to believe that many of these doubtful and closely allied forms have permanently retained their characters for a long time; for as long, as far as we know, as have good and true species. Practically, when a naturalist can unite by means of intermediate links any two forms, he treats the one as a variety of the other; ranking the most common, but sometimes the one first described, as the species, and the other as the variety.

Many years ago, when comparing, and seeing others compare, the birds from the closely neighbouring islands of the Galapagos archipelago, one with another, and with those from the American mainland, I was much struck how entirely vague and arbitrary is the distinction between species and varieties. On the islets of the little Madeira group there are many insects which are characterised as varieties in Mr. Wollaston's admirable work, but which would certainly be ranked as distinct species by many entomologists. Even Ireland has a few animals, now generally regarded as varieties, but which have been ranked as species by some zoologists. Several experienced ornithologists consider our British red grouse as only a strongly marked race of a Norwegian species, whereas the greater number rank it as an undoubted species peculiar to Great Britain. A wide distance between the homes of two doubtful forms leads many naturalists to rank them as distinct species; but what distance, it has been well asked, will suffice; if that between America and Europe is ample, will that between Europe and the Azores, or Madeira, or the Canaries, or between the several islets of these small archipelagos, be sufficient?

Certainly no clear line of demarcation has as yet been drawn between species and sub-species—that is, the forms which in the opinion of some naturalists come very near to, but do not quite arrive at, the rank of species: or, again, between sub-species and well-marked varieties, or between lesser varieties and individual differences. These differences blend into each other by an insensible series; and a series impresses the mind with the idea of an actual passage.

Hence I look at individual differences, though of small interest to the systematist, as of the highest importance for us, as being the first steps towards such slight varieties as are barely thought worth recording in works on natural history. And I look at varieties which are in any degree more distinct and permanent as steps towards more strongly marked and permanent varieties; and at the latter as leading to sub-species, and then to species. The passage from one stage of difference to another may, in many cases, be the simple result of the nature of the organism and of the different physical conditions to which it has long been exposed; but with respect to the more important and adaptive characters, the passage from one stage of difference to another may be safely attributed to the cumulative action of natural selection, hereafter to be explained, and to the effects

of the increased use or disuse of parts. A well-marked variety may therefore be called an incipient species; but whether this belief is justifiable must be judged by the weight of the various facts and considerations to be given throughout this work.

From these remarks it will be seen that I look at the term species as one arbitrarily given, for the sake of convenience, to a set of individuals closely resembling each other, and that it does not essentially differ from the term variety, which is given to less distinct and more fluctuating forms. The term variety, again, in comparison with mere individual differences, is also applied arbitrarily, for convenience' sake.

Wide-ranging, much diffused, and common Species vary most

Alphonse de Candolle and others have shown that plants which have very wide ranges generally present varieties; and this might have been expected, as they are exposed to diverse physical conditions, and as they come into competition (which, as we shall hereafter see, is an equally or more important circumstance) with different sets of organic beings. But my tables further show that, in any limited country, the species which are the most common, that is, abound most in individuals, and the species which are most widely diffused within their own country (and this is a different consideration from wide range, and to a certain extent from commonness) oftenest give rise to varieties sufficiently well marked to have been recorded in botanical works. Hence it is the most flourishing, or, as they may be called, the dominant species—those which range widely, are the most diffused in their own country, and are the most numerous in individuals—which oftenest produce well-marked varieties, or, as I consider them, incipient species. And this, perhaps, might have been anticipated; for, as varieties, in order to become in any degree permanent, necessarily have to struggle with the other inhabitants of the country, the species which are already dominant will be the most likely to yield offspring which, though in some slight degree modified, still inherit those advantages that enabled their parents to become dominant over their compatriots. In these remarks on predominance, it should be understood that reference is made only to the forms which come into competition with each other, and more especially to the members of the same genus or class having nearly similar habits of life. With respect to the number of individuals or commonness of species, the comparison of course relates only to the members of the same group. One of the higher plants may be said to be dominant if it be more numerous in individuals and more widely diffused than the other plants of the same country, which live under nearly the same conditions. A plant of this kind is not the less dominant because some conferva inhabiting the water or some parasitic fungus is infinitely more numerous in individuals and more widely diffused. But if the conferva or parasitic fungus exceeds its allies in the above respects, it will then be dominant within its own class.

Species of the Larger Genera in each Country vary more frequently than the Species of the Smaller Genera

From looking at species as only strongly marked and well-defined varieties, I was led to anticipate that the species of the larger genera in each country would oftener present varieties than the species of the smaller genera; for wherever many closely related species (*i.e.*, species of the same genus) have been formed, many varieties or incipient species ought, as a general rule, to be now forming. Where many large trees grow, we expect to find saplings. Where many species of a genus have been formed through variation, circumstances have been favourable for variation; and hence we might expect that the circumstances would generally be still favourable to variation. On the other hand, if we look at each species as a special act of creation, there is no apparent reason why more varieties should occur in a group having many species than in one having few.

To test the truth of this anticipation I have arranged the plants of twelve countries, and the coleopterous insects of two districts, into two nearly equal masses, the species of the larger genera on one side and those of the smaller genera on the other side, and it has invariably proved to be the case that a larger proportion of the species on the side of the larger genera presented varieties than on the side of the smaller genera. Moreover, the species of the large genera which present any varieties invariably present a larger average number of varieties than do the species of the small genera. Both these results follow when another division is made, and when all the least genera, with from only one to four species, are altogether excluded from the tables. These facts are of plain signification on the view that species are only strongly marked and permanent varieties; for wherever many species of the same genus have been formed, or where, if we may use the expression, the manufactory of species has been active, we ought generally to find the manufactory still in action, more especially as we have every reason to believe the process of manufacturing new species to be a slow one. And this certainly holds true if varieties be looked at as incipient species; for my tables clearly show as a general rule that, wherever many species of a genus have been formed, the species of that genus present a number of varieties, that is of incipient species, beyond the average. It is not that all large genera are now varying much, and are thus increasing in the number of their species, or that no small genera are now varying and increasing; for if this had been so, it would have been fatal to my theory; inasmuch as geology plainly tells us that small genera have in the lapse of time often increased greatly in size; and that large genera have often come to their maxima, decline, and disappeared. All that we want to show is that, when many species of a genus have been formed, on an average many are still forming; and this certainly holds good.

Many of the Species included within the Larger Genera resemble Varieties in being very closely, but unequally, related to each other, and in having restricted ranges

There are other relations between the species of large genera and their recorded varieties which deserve notice. We have seen that there is no infallible criterion by which to distinguish species and well-marked varieties; and when intermediate links have not been found between doubtful forms, naturalists are compelled to come to a determination by the amount of difference between them, judging by analogy whether or not the amount suffices to raise one or both to the rank of species. Hence the amount of difference is one very important criterion in settling whether two forms should be ranked as species or varieties. Now Fries has remarked in regard to plants, and Westwood in regard to insects, that in large genera the amount of difference between the species is often exceedingly small. I have endeavoured to test this numerically by averages, and, as far as my imperfect results go, they confirm the view. I have also consulted some sagacious and experienced observers, and, after deliberation, they concur in this view. In this respect, therefore, the species of the larger genera resemble varieties more than do the species of the smaller genera. Or the case may be put in another way, and it may be said that in the larger genera, in which a number of varieties or incipient species greater than the average are now manufacturing, many of the species already manufactured still to a certain extent resemble varieties, for they differ from each other by less than the usual amount of difference.

Moreover, the species of the larger genera are related to each other, in the same manner as the varieties of any one species are related to each other. No naturalist pretends that all the species of a genus are equally distinct from each other; they may generally be divided into sub-genera, or sections, or lesser groups. As Fries has well remarked, little groups of species are generally clustered like satellites around other species. And what are varieties but groups of forms, unequally related to each other, and clustered round certain forms—that is, round their parent species?

III. STRUGGLE FOR EXISTENCE

Before entering on the subject of this chapter, I must make a few preliminary remarks, to show how the struggle for existence bears on Natural Selection. It has been seen in the last chapter that amongst organic beings in a state of nature there is some individual variability: indeed I am not aware that this has ever been disputed. It is immaterial for us whether a multitude of doubtful forms be called species or sub-species or varieties; what rank, for instance, the two or three hundred doubtful forms of British plants are entitled to hold, if the existence of any well-marked varie-

ties be admitted. But the mere existence of individual variability and of some few well-marked varieties, though necessary as the foundation for the work, helps us but little in understanding how species arise in nature. How have all those exquisite adaptations of one part of the organisation to another part, and to the conditions of life, and of one organic being to another being, been perfected? We see these beautiful co-adaptations most plainly in the woodpecker and the mistletoe; and only a little less plainly in the humblest parasite which clings to the hairs of a quadruped or feathers of a bird; in the structure of the beetle which dives through the water; in the plumed seed which is wafted by the gentlest breeze; in short, we see beautiful adaptations everywhere and in every part of the organic world.

Again, it may be asked, how is it that varieties, which I have called incipient species, become ultimately converted into good and distinct species which in most cases obviously differ from each other far more than do the varieties of the same species? How do those groups of species, which constitute what are called distinct genera, and which differ from each other more than do the species of the same genus, arise? All these results, as we shall more fully see in the next chapter, follow from the struggle for life. Owing to this struggle, variations, however slight and from whatever cause proceeding, if they be in any degree profitable to the individuals of a species, in their infinitely complex relations to other organic beings and to their physical conditions of life, will tend to the preservation of such individuals, and will generally be inherited by the offspring. The offspring, also, will thus have a better chance of surviving, for, of the many individuals of any species which are periodically born, but a small number can survive. I have called this principle, by which each slight variation, if useful, is preserved, by the term Natural Selection, in order to mark its relation to man's power of selection. But the expression often used by Mr. Herbert Spencer of the Survival of the Fittest is more accurate, and is sometimes equally convenient. We have seen that man by selection can certainly produce great results, and can adapt organic beings to his own uses, through the accumulation of slight but useful variations, given to him by the hand of Nature. But Natural Selection, as we shall hereafter see, is a power incessantly ready for action, and is as immeasurably superior to man's feeble efforts as the works of Nature are to those of Art.

Nothing is easier than to admit in words the truth of the universal struggle for life, or more difficult—at least I have found it so—than constantly to bear this conclusion in mind. Yet unless it be thoroughly engrained in the mind, the whole economy of nature, with every fact on distribution, rarity, abundance, extinction, and variation, will be dimly seen or quite misunderstood. We behold the face of nature bright with gladness, we often see superabundance of food; we do not see, or we forget, that the birds which are idly singing round us mostly live on insects or seeds, and are thus constantly destroying life; or we forget how largely these songsters, or their eggs, or their nestlings, are destroyed by birds and beasts of prey; we do not always bear in mind that, though

food may be now superabundant, it is not so at all seasons of each recurring year.

The Term, Struggle for Existence, used in a large sense

I should premise that I use this term in a large and metaphorical sense including dependence of one being on another, and including (which is more important) not only the life of the individual, but success in leaving progeny. Two canine animals, in a time of dearth, may be truly said to struggle with each other which shall get food and live. But a plant on the edge of a desert is said to struggle for life against the drought, though more properly it should be said to be dependent on the moisture. A plant which annually produces a thousand seeds, of which only one of an average comes to maturity, may be more truly said to struggle with the plants of the same and other kinds which already clothe the ground. The mistletoe is dependent on the apple and a few other trees, but can only in a far-fetched sense be said to struggle with these trees, for, if too many of these parasites grow on the same tree, it languishes and dies. But several seedling mistletoes, growing close together on the same branch, may more truly be said to struggle with each other. As the mistletoe is disseminated by birds, its existence depends on them; and it may methodically be said to struggle with other fruit-bearing plants, in tempting the birds to devour and thus disseminate its seeds. In these several senses, which pass into each other, I use for convenience' sake the general term of Struggle for Existence.

Geometrical Ratio of Increase

A struggle for existence inevitably follows from the high rate at which all organic beings tend to increase. Every being, which during its natural lifetime produces several eggs or seeds, must suffer destruction during some period of its life, and during some season or occasional year, otherwise, on the principle of geometrical increase, its numbers would quickly become so inordinately great that no country could support the product. Hence, as more individuals are produced than can possibly survive, there must in every case be a struggle for existence, either one individual with another of the same species, or with the individuals of distinct species, or with the physical conditions of life. It is the doctrine of Malthus applied with manifold force to the whole animal and vegetable kingdoms; for in this case there can be no artificial increase of food, and no prudential restraint from marriage. Although some species may be now increasing, more or less rapidly, in numbers, all cannot do so, for the world would not hold them.

There is no exception to the rule that every organic being naturally increases at so high a rate that, if not destroyed, the earth would soon be covered by the progeny of a single pair. Even slow-breeding man has

doubled in twenty-five years, and at this rate, in less than a thousand years, there would literally not be standing room for his progeny. Linnæus has calculated that if an annual plant produced only two seeds—and there is no plant so unproductive as this—and their seedlings next year produced two, and so on, then in twenty years there should be a million plants. The elephant is reckoned the slowest breeder of all known animals, and I have taken some pains to estimate its probable minimum rate of natural increase; it will be safest to assume that it begins breeding when thirty years old, and goes on breeding till ninety years old, bringing forth six young in the interval, and surviving till one hundred years old; if this be so, after a period of from 740 to 750 years there would be nearly nineteen million elephants alive, descended from the first pair.

The only difference between organisms which annually produce eggs or seeds by the thousand and those which produce extremely few is that the slow breeders would require a few more years to people, under favourable conditions, a whole district, let it be ever so large. The condor lays a couple of eggs and the ostrich a score, and yet in the same country the condor may be the more numerous of the two; the Fulmar petrel lays but one egg, yet it is believed to be the most numerous bird in the world. One fly deposits hundreds of eggs, and another, like the hippobosca, a single one; but this difference does not determine how many individuals of the two species can be supported in a district. A large number of eggs is of some importance to those species which depend on a fluctuating amount of food, for it allows them rapidly to increase in number. But the real importance of a large number of eggs or seeds is to make up for much destruction at some period of life; and this period in the great majority of cases is an early one. If an animal can in any way protect its own eggs or young, a small number may be produced, and yet the average stock be fully kept up; but if many eggs or young are destroyed, many must be produced, or the species will become extinct. It would suffice to keep up the full number of a tree, which lived on an average for a thousand years, if a single seed were produced once in a thousand years, supposing that this seed were never destroyed, and could be ensured to germinate in a fitting place. So that, in all cases, the average number of any animal or plant depends only indirectly on the number of its eggs or seeds.

In looking at Nature, it is most necessary to keep the foregoing considerations always in mind—never to forget that every single organic being may be said to be striving to the utmost to increase in numbers; that each lives by a struggle at some period of its life; that heavy destruction inevitably falls either on the young or old, during each generation or at recurrent intervals. Lighten any check, mitigate the destruction ever so little, and the number of the species will almost instantaneously increase to any amount.

Complex Relations of all Animals and Plants to each other in the Struggle for Existence

Many cases are on record showing how complex and unexpected are the checks and relations between organic beings, which have to struggle together in the same country. I will give only a single instance, which, though a simple one, interested me. In Staffordshire, on the estate of a relation, where I had ample means of investigation, there was a large and extremely barren heath, which had never been touched by the hand of man; but several hundred acres of exactly the same nature had been enclosed twenty-five years previously and planted with Scotch fir. The change in the native vegetation of the planted part of the heath was most remarkable, more than is generally seen in passing from one quite different soil to another: not only the proportional numbers of the heath plants were wholly changed, but twelve species of plants (not counting grasses and carices) flourished in the plantations, which could not be found on the heath. The effect on the insects must have been still greater, for six insectivorous birds were very common in the plantations, which were not to be seen on the heath; and the heath was frequented by two or three distinct insectivorous birds. Here we see how potent has been the effect of the introduction of a single tree, nothing whatever else having been done, with the exception of the land having been enclosed, so that cattle could not enter. But how important an element enclosure is, I plainly saw near Farnham, in Surrey. Here there are extensive heaths, with a few clumps of old Scotch firs on the distant hilltops: within the last ten years large spaces have been enclosed, and self-sown firs are now springing up in multitudes, so close together that all cannot live. When I ascertained that these young trees had not been sown or planted, I was so much surprised at their numbers that I went to several points of view, whence I could examine hundreds of acres of the unenclosed heath, and literally I could not see a single Scotch fir, except the old planted clumps. But on looking closely between the stems of the heath, I found a multitude of seedlings and little trees which had been perpetually browsed down by the cattle. In one square yard, at a point some hundred yards distant from one of the old clumps, I counted thirty-two little trees; and one of them, with twenty-six rings of growth, had, during many years, tried to raise its head above the stems of the heath, and had failed. No wonder that, as soon as the land was enclosed, it became thickly clothed with vigorously growing young firs. Yet the heath was so extremely barren and so extensive that no one would ever have imagined that cattle would have so closely and effectually searched it for food.

Here we see that cattle absolutely determine the existence of the Scotch fir; but in several parts of the world insects determine the existence of cattle. Perhaps Paraguay offers the most curious instance of this; for here neither cattle nor horses nor dogs have ever run wild, though

they swarm southward and northward in a feral state; and Azara and Rengger have shown that this is caused by the greater number in Paraguay of a certain fly, which lays its eggs in the navels of these animals when first born. The increase of these flies, numerous as they are, must be habitually checked by some means, probably by other parasitic insects. Hence, if certain insectivorous birds were to decrease in Paraguay, the parasitic insects would probably increase; and this would lessen the number of the navel-frequenting flies—then cattle and horses would become feral, and this would certainly greatly alter (as indeed I have observed in parts of South America) the vegetation: this again would largely affect the insects; and this, as we have just seen in Staffordshire, the insectivorous birds, and so onwards in ever-increasing circles of complexity. Not that under nature the relations will ever be as simple as this. Battle within battle must be continually recurring with varying success; and yet in the long run the forces are so nicely balanced that the face of nature remains for long periods of time uniform, though assuredly the merest trifle would give the victory to one organic being over another. Nevertheless, so profound is our ignorance, and so high our presumption, that we marvel when we hear of the extinction of an organic being; and as we do not see the cause, we invoke cataclysms to desolate the world, or invent laws on the duration of the forms of life!

The dependency of one organic being on another, as of a parasite on its prey, lies generally between beings remote in the scale of nature. This is likewise sometimes the case with those which may be strictly said to struggle with each other for existence, as in the case of locusts and grass-feeding quadrupeds. But the struggle will almost invariably be most severe between the individuals of the same species, for they frequent the same districts, require the same food, and are exposed to the same dangers. In the case of varieties of the same species, the struggle will generally be almost equally severe, and we sometimes see the contest soon decided: for instance, if several varieties of wheat be sown together, and the mixed seed be resown, some of the varieties which best suit the soil or climate, or are naturally the most fertile, will beat the others and so yield more seed, and will consequently in a few years supplant the other varieties.

As the species of the same genus usually have, though by no means invariably, much similarity in habits and constitution, and always in structure, the struggle will generally be more severe between them, if they come into competition with each other, than between the species of distinct genera. We see this in the recent extension over parts of the United States of one species of swallow having caused the decrease of another species. The recent increase of the missel thrush in parts of Scotland has caused the decrease of the song thrush. How frequently we hear of one species of rat taking the place of another species under the most different climates! In Russia the small Asiatic cockroach has everywhere driven before it its great congener. In Australia the imported hive bee is rapidly exterminating the small, stingless native bee. One species of charlock has been known to supplant another species; and so in other cases. We can

dimly see why the competition should be most severe between allied forms, which fill nearly the same place in the economy of nature; but probably in no one case could we precisely say why one species has been victorious over another in the great battle of life.

IV. NATURAL SELECTION; OR THE SURVIVAL OF THE FITTEST

How will the struggle for existence, briefly discussed in the last chapter, act in regard to variation? Can the principle of selection, which we have seen is so potent in the hands of man, apply under nature? I think we shall see that it can act most efficiently. Let the endless number of slight variations and individual differences occurring in our domestic productions, and, in a lesser degree, in those under nature, be borne in mind; as well as the strength of the hereditary tendency. Under domestication, it may be truly said that the whole organisation becomes in some degree plastic. But the variability, which we almost universally meet with in our domestic productions, is not directly produced, as Hooker and Asa Gray have well remarked, by man; he can neither originate varieties nor prevent their occurrence; he can preserve and accumulate such as do occur. Unintentionally he exposes organic beings to new and changing conditions of life, and variability ensues; but similar changes of conditions might and do occur under nature. Let it also be borne in mind how infinitely complex and close-fitting are the mutual relations of all organic beings to each other and to their physical conditions of life; and consequently what infinitely varied diversities of structure might be of use to each being under changing conditions of life. Can it, then, be thought improbable, seeing that variations useful to man have undoubtedly occurred, that other variations useful in some way to each being in the great and complex battle of life should occur in the course of many successive generations? If such do occur, can we doubt (remembering that many more individuals are born than can possibly survive) that individuals having any advantage, however slight, over others, would have the best chance of surviving and of procreating their kind? On the other hand, we may feel sure that any variation in the least degree injurious would be rigidly destroyed. This preservation of favourable individual differences and variations, and the destruction of those which are injurious, I have called Natural Selection, or the Survival of the Fittest.

We shall best understand the probable course of natural selection by taking the case of a country undergoing some slight physical change, for instance, of climate. The proportional numbers of its inhabitants will almost immediately undergo a change, and some species will probably become extinct. We may conclude, from what we have seen of the intimate and complex manner in which the inhabitants of each country are bound together, that any change in the numerical proportions of the inhabitants, independently of the change of climate itself, would seriously affect the

others. If the country were open on its borders, new forms would certainly immigrate, and this would likewise seriously disturb the relations of some of the former inhabitants. Let it be remembered how powerful the influence of a single introduced tree or mammal has been shown to be. But in the case of an island, or of a country partly surrounded by barriers, into which new and better adapted forms could not freely enter, we should then have places in the economy of nature which would assuredly be better filled up, if some of the original inhabitants were in some manner modified; for, had the area been open to immigration, these same places would have been seized on by intruders. In such cases, slight modifications, which in any way favoured the individuals of any species, by better adapting them to their altered conditions, would tend to be preserved; and natural selection would have free scope for the work of improvement.

As man can produce, and certainly has produced, a great result by his methodical and unconscious means of selection, what may not natural selection effect? Man selects only for his own good: Nature only for that of the being which she tends. Every selected character is fully exercised by her, as is implied by the fact of their selection. Man keeps the natives of many climates in the same country; he seldom exercises each selected character in some peculiar and fitting manner; he feeds a long- and a short-beaked pigeon on the same food; he does not exercise a long-backed or long-legged quadruped in any peculiar manner; he exposes sheep with long and short wool to the same climate. He does not allow the most vigorous males to struggle for the females. He does not rigidly destroy all inferior animals, but protects during each varying season, as far as lies in his power, all his productions. He often begins his selection by some half-monstrous form; or at least by some modification prominent enough to catch the eye or to be plainly useful to him. Under nature, the slightest differences of structure or constitution may well turn the nicely balanced scale in the struggle for life, and so be preserved. How fleeting are the wishes and efforts of man! how short his time! and consequently how poor will be his results, compared with those accumulated by Nature during whole geological periods! Can we wonder, then, that Nature's productions should be far "truer" in character than man's productions; that they should be infinitely better adapted to the most complex conditions of life, and should plainly bear the stamp of far higher workmanship?

It may metaphorically be said that natural selection is daily and hourly scrutinising, throughout the world, the slightest variations; rejecting those that are bad, preserving and adding up all that are good; silently and insensibly working, *whenever and wherever opportunity offers,* at the improvement of each organic being in relation to its organic and inorganic conditions of life. We see nothing of these slow changes in progress, until the hand of time has marked the lapse of ages, and then so imperfect is our view into long-past geological ages that we see only that the forms of life are now different from what they formerly were.

Although natural selection can act only through and for the good of

each being, yet characters and structures, which we are apt to consider as of very trifling importance, may thus be acted on. When we see leaf-eating insects green, and bark feeders mottled grey; the alpine ptarmigan white in winter, the red grouse the colour of heather, we must believe that these tints are of service to these birds and insects in preserving them from danger. Grouse, if not destroyed at some period of their lives, would increase in countless numbers; they are known to suffer largely from birds of prey; and hawks are guided by eyesight to their prey—so much so that on parts of the Continent persons are warned not to keep white pigeons, as being the most liable to destruction. Hence natural selection might be effective in giving the proper colour to each kind of grouse, and in keeping that colour, when once acquired, true and constant. Nor ought we to think that the occasional destruction of an animal of any particular colour would produce little effect: we should remember how essential it is in a flock of white sheep to destroy a lamb with the faintest trace of black. We have seen how the colour of the hogs, which feed on the "paint-root" in Virginia, determines whether they shall live or die.

As we see that those variations which, under domestication, appear at any particular period of life tend to reappear in the offspring at the same period;—for instance, in the shape, size, and flavour of the seeds of the many varieties of our culinary and agricultural plants; in the caterpillar and cocoon stages of the varieties of the silkworm; in the eggs of poultry, and in the colour of the down of their chickens; in the horns of our sheep and cattle when nearly adult;—so in a state of nature natural selection will be enabled to act on and modify organic beings at any age, by the accumulation of variations profitable at that age, and by their inheritance at a corresponding age. If it profit a plant to have its seeds more and more widely disseminated by the wind, I can see no greater difficulty in this being effected through natural selection than in the cotton planter increasing and improving by selection the down in the pods on his cotton trees. Natural selection may modify and adapt the larva of an insect to a score of contingencies, wholly different from those which concern the mature insect; and these modifications may effect, through correlation, the structure of the adult. So, conversely, modifications in the adult may affect the structure of the larva; but in all cases natural selection will ensure that they shall not be injurious: for if they were so, the species would become extinct.

Natural selection will modify the structure of the young in relation to the parent, and of the parent in relation to the young. In social animals it will adapt the structure of each individual for the benefit of the whole community; if the community profits by the selected change. What natural selection cannot do is to modify the structure of one species, without giving it any advantage, for the good of another species; and though statements to this effect may be found in works of natural history, I cannot find one case which will bear investigation. A structure used only once in an animal's life, if of high importance to it, might be modified to any extent by natural selection; for instance, the great jaws possessed by certain insects, used exclusively for opening the cocoon—or the hard tip to

the beak of unhatched birds, used for breaking the egg. It has been asserted that of the best short-beaked tumbler pigeons a greater number perish in the egg than are able to get out of it; so that fanciers assist in the act of hatching. Now if nature had to make the beak of a full-grown pigeon very short for the bird's own advantage, the process of modification would be very slow, and there would be simultaneously the most rigorous selection of all the young birds within the egg, which had the most powerful and hardest beaks, for all with weak beaks would inevitably perish; or, more delicate and more easily broken shells might be selected, the thickness of the shell being known to vary like every other structure.

Illustrations of the Action of Natural Selection, or the Survival of the Fittest

In order to make it clear how, as I believe, natural selection acts, I must beg permission to give one or two imaginary illustrations. Let us take the case of a wolf, which preys on various animals, securing some by craft, some by strength, and some by fleetness; and let us suppose that the fleetest prey, a deer for instance, had from any change in the country increased in numbers, or that other prey had decreased in numbers, during that season of the year when the wolf was hardest pressed for food. Under such circumstances the swiftest and slimmest wolves would have the best chance of surviving and so be preserved or selected—provided always that they retained strength to master their prey at this or some other period of the year, when they were compelled to prey on other animals. I can see no more reason to doubt that this would be the result than that man should be able to improve the fleetness of his greyhounds by careful and methodical selection, or by that kind of unconscious selection which follows from each man trying to keep the best dogs without any thought of modifying the breed. I may add that, according to Mr. Pierce, there are two varieties of the wolf inhabiting the Catskill Mountains, in the United States, one with a light greyhound-like form, which pursues deer, and the other more bulky, with shorter legs, which more frequently attacks the shepherd's flocks.

It may be worth while to give another and more complex illustration of the action of natural selection. Certain plants excrete sweet juice, apparently for the sake of eliminating something injurious from the sap: this is effected, for instance, by glands at the base of the stipules in some Leguminosæ, and at the backs of the leaves of the common laurel. This juice, though small in quantity, is greedily sought by insects; but their visits do not in any way benefit the plant. Now, let us suppose that the juice or nectar was excreted from the inside of the flowers of a certain number of plants of any species. Insects in seeking the nectar would get dusted with pollen, and would often transport it from one flower to another. The flowers of two distinct individuals of the same species would thus get crossed; and the act of crossing, as can be fully proved, gives rise to vigorous seed-

lings which consequently would have the best chance of flourishing and surviving. The plants which produced flowers with the largest glands or nectaries, excreting most nectar, would oftenest be visited by insects, and would oftenest be crossed; and so in the long run would gain the upper hand and form a local variety. The flowers, also, which had their stamens and pistils placed, in relation to the size and habits of the particular insects which visited them, so as to favour in any degree the transportal of the pollen, would likewise be favoured.

Let us now turn to the nectar-feeding insects; we may suppose the plant, of which we have been slowly increasing the nectar by continued selection, to be a common plant; and that certain insects depended in main part on its nectar for food. I could give many facts showing how anxious bees are to save time: for instance, their habit of cutting holes and sucking the nectar at the bases of certain flowers, which, with a very little more trouble, they can enter by the mouth. Bearing such facts in mind, it may be believed that under certain circumstances individual differences in the curvature or length of the proboscis, &c., too slight to be appreciated by us, might profit a bee or other insect, so that certain individuals would be able to obtain their food more quickly than others; and thus the communities to which they belonged would flourish and throw off many swarms inheriting the same peculiarities. The tubes of the corolla of the common red and incarnate clovers (Trifolium pratense and incarnatum) do not on a hasty glance appear to differ in length; yet the hive bee can easily suck the nectar out of the incarnate clover, but not out of the common red clover, which is visited by humblebees alone; so that whole fields of red clover offer in vain an abundant supply of precious nectar to the hive bee. That this nectar is much liked by the hive bee is certain; for I have repeatedly seen, but only in the autumn, many hive bees sucking the flowers through holes bitten in the base of the tube by humblebees. Thus, in a country where this kind of clover abounded, it might be a great advantage to the hive bee to have a slightly longer or differently constructed proboscis. On the other hand, as the fertility of this clover absolutely depends on bees visiting the flowers, if humblebees were to become rare in any country, it might be a great advantage to the plant to have a shorter or more deeply divided corolla, so that the hive bees should be enabled to suck its flowers. Thus I can understand how a flower and a bee might slowly become, either simultaneously or one after the other, modified and adapted to each other in the most perfect manner, by the continued preservation of all the individuals which presented slight deviations of structure mutually favourable to each other.

I am well aware that this doctrine of natural selection, exemplified in the above imaginary instances, is open to the same objections which were first urged against Sir Charles Lyell's noble views on "the modern changes of the earth, as illustrative of geology"; but we now seldom hear the agencies which we see still at work, spoken of as trifling or insignificant, when used in explaining the excavation of the deepest valleys or the formation of long lines of inland cliffs. Natural selection acts only by the preserva-

tion and accumulation of small inherited modifications, each profitable to the preserved being; and as modern geology has almost banished such views as the excavation of a great valley by a single diluvial wave, so will natural selection banish the belief of the continued creation of new organic beings, or of any great and sudden modification in their structure.

Circumstances favourable for the production of new forms through Natural Selection

This is an extremely intricate subject. A great amount of variability, under which term individual differences are always included, will evidently be favourable. A large number of individuals, by giving a better chance within any given period for the appearance of profitable variations, will compensate for a lesser amount of variability in each individual, and is, I believe, a highly important element of success. Though Nature grants long periods of time for the work of natural selection, she does not grant an indefinite period; for as all organic beings are striving to seize on each place in the economy of nature, if any one species does not become modified and improved in a corresponding degree with its competitors, it will be exterminated. Unless favourable variations be inherited by some at least of the offspring, nothing can be effected by natural selection. The tendency to reversion may often check or prevent the work; but as this tendency has not prevented man from forming by selection numerous domestic races, why should it prevail against natural selection?

Intercrossing will chiefly affect those animals which unite for each birth and wander much, and which do not breed at a very quick rate. Hence with animals of this nature, for instance, birds, varieties will generally be confined to separated countries; and this I find to be the case. With hermaphrodite organisms which cross only occasionally, and likewise with animals which unite for each birth, but which wander little and can increase at a rapid rate, a new and improved variety might be quickly formed on any one spot, and might there maintain itself in a body and afterwards spread, so that the individuals of the new variety would chiefly cross together. On this principle, nurserymen always prefer saving seed from a large body of plants, as the chance of intercrossing is thus lessened.

Isolation, also, is an important element in the modification of species through natural selection. In a confined or isolated area, if not very large, the organic and inorganic conditions of life will generally be almost uniform; so that natural selection will tend to modify all the varying individuals of the same species in the same manner. Intercrossing with the inhabitants of the surrounding districts will, also, be thus prevented. The importance of isolation is likewise great in preventing, after any physical change in the conditions, such as of climate, elevation of the land, &c., the immigration of better adapted organisms; and thus new places in the natural economy of the district will be left open to be filled up by the modification of the old inhabitants. Lastly, isolation will give time for a

new variety to be improved at a slow rate; and this may sometimes be of much importance.

Although isolation is of great importance in the production of new species, on the whole I am inclined to believe that largeness of area is still more important, especially for the production of species which shall prove capable of enduring for a long period, and of spreading widely. Throughout a great and open area, not only will there be a better chance of favourable variations, arising from the large number of individuals of the same species there supported, but the conditions of life are much more complex from the large number of already existing species; and if some of these many species become modified and improved, others will have to be improved in a corresponding degree, or they will be exterminated. Each new form, also, as soon as it has been much improved, will be able to spread over the open and continuous area, and will thus come into competition with many other forms. Moreover, great areas, though now continuous, will often, owing to former oscillations of level, have existed in a broken condition; so that the good effects of isolation will generally, to a certain extent, have concurred.

To sum up, as far as the extreme intricacy of the subject permits, the circumstances favourable and unfavourable for the production of new species through natural selection. I conclude that for terrestrial productions a large continental area, which has undergone many oscillations of level, will have been the most favourable for the production of many new forms of life, fitted to endure for a long time and to spread widely. Whilst the area existed as a continent, the inhabitants will have been numerous in individuals and kinds, and will have been subjected to severe competition. When converted by subsidence into large separate islands, there will still have existed many individuals of the same species on each island: intercrossing on the confines of the range of each new species will have been checked: after physical changes of any kind, immigration will have been prevented, so that new places in the polity of each island will have had to be filled up by the modification of the old inhabitants; and time will have been allowed for the varieties in each to become well modified and perfected. When, by renewed elevation, the islands were reconverted into a continental area, there will again have been very severe competition: the most favoured or improved varieties will have been enabled to spread: there will have been much extinction of the less improved forms, and the relative proportional numbers of the various inhabitants of the reunited continent will again have been changed; and again there will have been a fair field for natural selection to improve still further the inhabitants, and thus to produce new species.

Slow though the process of selection may be, if feeble man can do much by artificial selection, I can see no limit to the amount of change, to the beauty and complexity of the coadaptations between all organic beings, one with another and with their physical conditions of life, which may have been effected in the long course of time through nature's power of selection, that is, by the survival of the fittest.

Divergence of Character

The principle, which I have designated by this term, is of high importance, and explains, as I believe, several important facts. In the first place, varieties, even strongly marked ones, though having somewhat of the character of species—as is shown by the hopeless doubts in many cases how to rank them—yet certainly differ far less from each other than do good and distinct species. Nevertheless, according to my view, varieties are species in the process of formation, or are, as I have called them, incipient species. How, then, does the lesser difference between varieties become augmented into the greater difference between species? That this does habitually happen, we must infer from most of the innumerable species throughout nature presenting well-marked differences; whereas varieties, the supposed prototypes and parents of future well-marked species, present slight and ill-defined differences. Mere chance, as we may call it, might cause one variety to differ in some character from its parents, and the offspring of this variety again to differ from its parent in the very same character and in a greater degree; but this alone would never account for so habitual and large a degree of difference as that between the species of the same genus.

As has always been my practice, I have sought light on this head from our domestic productions. We shall here find something analogous. It will be admitted that the production of races so different as Shorthorn and Hereford cattle, race and cart horses, the several breeds of pigeons, &c., could never have been effected by the mere chance accumulation of similar variations during many successive generations. In practice, a fancier is, for instance, struck by a pigeon having a slightly shorter beak; another fancier is struck by a pigeon having a rather longer beak; and on the acknowledged principle that "fanciers do not and will not admire a medium standard, but like extremes," they both go on (as has actually occurred with the sub-breeds of the tumbler pigeon) choosing and breeding from birds with longer and longer beaks, or with shorter and shorter beaks. Again, we may suppose that at an early period of history, the men of one nation or district required swifter horses, whilst those of another required stronger and bulkier horses. The early differences would be very slight; but, in the course of time, from the continued selection of swifter horses in the one case, and of stronger ones in the other, the differences would become greater, and would be noted as forming two sub-breeds. Ultimately, after the lapse of centuries, these sub-breeds would become converted into two well-established and distinct breeds. As the differences became greater, the inferior animals with intermediate characters, being neither swift nor very strong, would not have been used for breeding, and will thus have tended to disappear. Here, then, we see in man's productions the action of what may be called the principle of divergence, causing differences, at first barely appreciable, steadily to increase, and the breeds

to diverge in character, both from each other and from their common parent.

But how, it may be asked, can any analogous principle apply in nature? I believe it can and does apply most efficiently (though it was a long time before I saw how), from the simple circumstance that the more diversified the descendants from any one species become in structure, constitution, and habits, by so much will they be better enabled to seize on many and widely diversified places in the polity of nature, and so be enabled to increase in numbers.

We can clearly discern this in the case of animals with simple habits. Take the case of a carnivorous quadruped, of which the number that can be supported in any country has long ago arrived at its full average. If its natural power of increase be allowed to act, it can succeed in increasing (the country not undergoing any change in conditions) only by its varying descendants seizing on places at present occupied by other animals: some of them, for instance, being enabled to feed on new kinds of prey, either dead or alive; some inhabiting new stations, climbing trees, frequenting water, and some perhaps becoming less carnivorous. The more diversified in habits and structure the descendants of our carnivorous animals become, the more places they will be enabled to occupy.

The advantage of diversification of structure in the inhabitants of the same region is, in fact, the same as that of the physiological division of labour in the organs of the same individual body—a subject so well elucidated by Milne Edwards. No physiologist doubts that a stomach adapted to digest vegetable matter alone, or flesh alone, draws most nutriment from these substances. So in the general economy of any land, the more widely and perfectly the animals and plants are diversified for different habits of life, so will a greater number of individuals be capable of there supporting themselves. A set of animals, with their organisation but little diversified, could hardly compete with a set more perfectly diversified in structure. It may be doubted, for instance, whether the Australian marsupials, which are divided into groups differing but little from each other, and feebly representing, as Mr. Waterhouse and others have remarked, our carnivorous, ruminant, and rodent mammals, could successfully compete with these well-developed orders. In the Australian mammals, we see the process of diversification in an early and incomplete stage of development.

The Probable Effects of the Action of Natural Selection through Divergence of Character and Extinction, on the Descendants of a Common Ancestor.

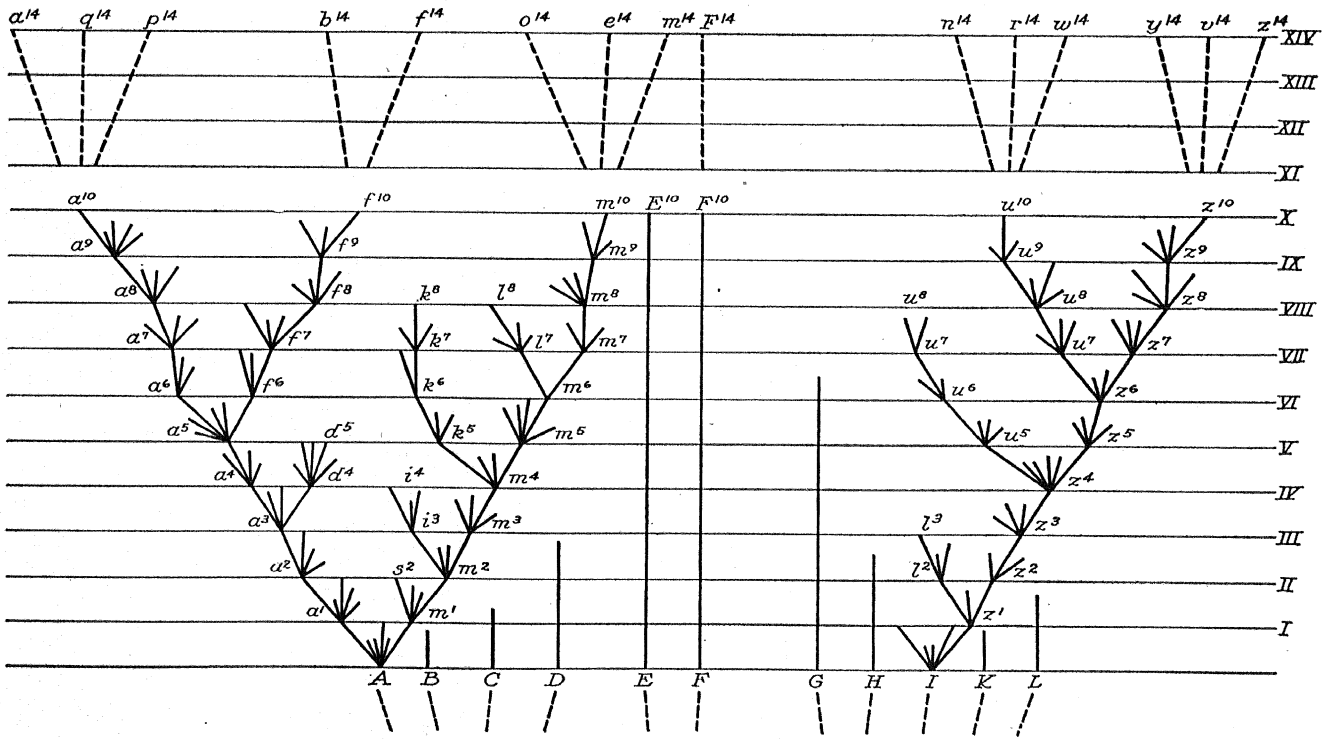
After the foregoing discussion, which has been much compressed, we may assume that the modified descendants of any one species will succeed so much the better as they become more diversified in structure, and are thus enabled to encroach on places occupied by other beings. Now let us

see how this principle of benefit being derived from divergence of character, combined with the principles of natural selection and of extinction, tends to act.

The accompanying diagram will aid us in understanding this rather perplexing subject. Let A to L represent the species of a genus large in its own country; these species are supposed to resemble each other in unequal degrees, as is so generally the case in nature, and as is represented in the diagram by the letters standing at unequal distances. I have said a large genus because, as we saw in the second chapter, on an average more species vary in large genera than in small genera; and the varying species of the large genera present a greater number of varieties. We have, also, seen that the species which are the commonest and the most widely diffused vary more than do the rare and restricted species. Let (A) be a common, widely diffused, and varying species, belonging to a genus large in its own country. The branching and diverging dotted lines of unequal lengths proceeding from (A) may represent its varying offspring. The variations are supposed to be extremely slight, but of the most diversified nature; they are not supposed all to appear simultaneously, but often after long intervals of time; nor are they all supposed to endure for equal periods. Only those variations which are in some way profitable will be preserved or naturally selected. And here the importance of the principle of benefit derived from divergence of character comes in; for this will generally lead to the most different or divergent variations (represented by the outer dotted lines) being preserved and accumulated by natural selection. When a dotted line reaches one of the horizontal lines, and is there marked by a small numbered letter, a sufficient amount of variation is supposed to have been accumulated to form it into a fairly well-marked variety, such as would be thought worthy of record in a systematic work.

The intervals between the horizontal lines in the diagram may represent each a thousand or more generations. After a thousand generations, species (A) is supposed to have produced two fairly well-marked varieties, namely a^1 and m^1. These two varieties will generally still be exposed to the same conditions which made their parents variable, and the tendency to variability is in itself hereditary; consequently they will likewise tend to vary, and commonly in nearly the same manner as did their parents. Moreover, these two varieties, being only slightly modified forms, will tend to inherit those advantages which made their parent (A) more numerous than most of the other inhabitants of the same country; they will also partake of those more general advantages which made the genus to which the parent species belonged a large genus in its own country. And all these circumstances are favourable to the production of new varieties.

If, then, these two varieties be variable, the most divergent of their variations will generally be preserved during the next thousand generations. And after this interval, variety a^1 is supposed in the diagram to have produced variety a^2, which will, owing to the principle of divergence, differ more from (A) than did variety a^1. Variety m^1 is supposed


a14 q14 p14 b14 f14 o14 e14 m14 F14 n14 r14 w14 y14 v14 z14
XIV XIII XII XI X IX VIII VII VI V IV III II I
a10 f10 m10 E10 F10 u10 z10
a9 f9 m9 u9 z9
a8 f8 k8 l8 m8 u8 z8
a7 f7 k7 l7 m7 u7 z7
a6 f6 k6 m6 u6 z6
a5 d5 k5 m5 u5 z5
a4 d4 i4 m4 z4
a3 i3 m3 l3 z3
a2 s2 m2 l2 z2
a1 m1 z1
A B C D E F G H I K L

to have produced two varieties, namely m^2 and s^2, differing from each other, and more considerably from their common parent (A). We may continue the process by similar steps for any length of time; some of the varieties, after each thousand generations, producing only a single variety, but in a more and more modified condition, some producing two or three varieties, and some failing to produce any. Thus the varieties or modified descendants of the common parent (A) will generally go on increasing in number and diverging in character. In the diagram the process is represented up to the ten thousandth generation, and under a condensed and simplified form up to the fourteen thousandth generation.

As all the modified descendants from a common and widely diffused species, belonging to a large genus, will tend to partake of the same advantages which made their parent successful in life, they will generally go on multiplying in number as well as diverging in character: this is represented in the diagram by the several divergent branches proceeding from (A). The modified offspring from the later and more highly improved branches in the lines of descent will, it is probable, often take the place of, and so destroy, the earlier and less improved branches: this is represented in the diagram by some of the lower branches not reaching to the upper horizontal lines. In some cases no doubt the process of modification will be confined to a single line of descent and the number of modified descendants will not be increased; although the amount of divergent modification may have been augmented. This case would be represented in the diagram, if all the lines proceeding from (A) were removed, excepting that from a^1 to a^{10}. In the same way the English race horse and English pointer have apparently both gone on slowly diverging in character from their original stocks, without either having given off any fresh branches or races.

After ten thousand generations, species (A) is supposed to have produced three forms, a^{10}, f^{10}, and m^{10}, which, from having diverged in character during the successive generations, will have come to differ largely, but perhaps unequally, from each other and from their common parent. If we suppose the amount of change between each horizontal line in our diagram to be excessively small, these three forms may still be only well-marked varieties; but we have only to suppose the steps in the process of modification to be more numerous or greater in amount, to convert these three forms into doubtful or at least into well-defined species. Thus the diagram illustrates the steps by which the small differences distinguishing varieties are increased into the larger differences distinguishing species. By continuing the same process for a greater number of generations (as shown in the diagram in a condensed and simplified manner), we get eight species, marked by the letters between a^{14} and m^{14}, all descended from (A). Thus, as I believe, species are multiplied and genera are formed.

In a large genus it is probable that more than one species would vary. In the diagram I have assumed that a second species (I) has produced, by analogous steps, after ten thousand generations, either two well-marked

varieties (w^{10} and z^{10}) or two species, according to the amount of change supposed to be represented between the horizontal lines. After fourteen thousand generations, six new species, marked by the letters n^{14} to z^{14}, are supposed to have been produced. In any genus, the species which are already very different in character from each other will generally tend to produce the greatest number of modified descendants; for these will have the best chance of seizing on new and widely different places in the polity of nature: hence in the diagram I have chosen the extreme species (A), and the nearly extreme species (I), as those which have largely varied and have given rise to new varieties and species. The other nine species (marked by capital letters) of our original genus may for long but unequal periods continue to transmit unaltered descendants; and this is shown in the diagram by the dotted lines unequally prolonged upwards.

But during the process of modification, represented in the diagram, another of our principles, namely that of extinction, will have played an important part. As in each fully stocked country natural selection necessarily acts by the selected form having some advantage in the struggle for life over other forms, there will be a constant tendency in the improved descendants of any one species to supplant and exterminate in each stage of descent their predecessors and their original progenitor. For it should be remembered that the competition will generally be most severe between those forms which are most nearly related to each other in habits, constitution, and structure. Hence all the intermediate forms between the earlier and later states, that is between the less and more improved states of the same species, as well as the original parent species itself, will genrally tend to become extinct. So it probably will be with many whole collateral lines of descent, which will be conquered by later and improved lines. If, however, the modified offspring of a species get into some distinct country, or become quickly adapted to some quite new station, in which offspring and progenitor do not come into competition, both may continue to exist.

If, then, our diagram be assumed to represent a considerable amount of modification, species (A) and all the earlier varieties will have become extinct, being replaced by eight new species (a^{14} to m^{14}); and species (I) will be replaced by six (n^{14} to z^{14}) new species.

But we may go further than this. The original species of our genus were supposed to resemble each other in unequal degrees, as is so generally the case in nature; species (A) being more nearly related to B, C, and D than to the other species; and species (I) more to G, H, K, L, than to the others. These two species (A) and (I) were also supposed to be very common and widely diffused species, so that they must originally have had some advantage over most of the other species of the genus. Their modified descendants, fourteen in number at the fourteen thousandth generation, will probably have inherited some of the same advantages: they have also been modified and improved in a diversified manner at each stage of descent, so as to have become adapted to many related places in the natural economy of their country. It seems, therefore, ex-

tremely probable that they will have taken the places of, and thus exterminated not only their parents (A) and (I), but likewise some of the original species which were most nearly related to their parents. Hence very few of the original species will have transmitted offspring to the fourteen thousandth generation. We may suppose that only one, (F), of the two species (E and F) which were least closely related to the other nine original species has transmitted descendants to this late stage of descent.

The new species in our diagram descended from the original eleven species will now be fifteen in number. Owing to the divergent tendency of natural selection, the extreme amount of difference in character between species a^{14} and z^{14} will be much greater than that between the most distinct of the original eleven species. The new species, moreover, will be allied to each other in a widely different manner. Of the eight descendants from (A) the three marked a^{14}, q^{14}, p^{14}, will be nearly related from having recently branched off from a^{10}; b^{14} and f^{14}, from having diverged at an earlier period from a^5, will be in some degree distinct from the three first-named species; and lastly, o^{14}, e^{14}, and m^{14} will be nearly related one to the other, but, from having diverged at the first commencement of the process of modification, will be widely different from the other five species, and may constitute a sub-genus or a distinct genus.

The six descendants from (I) will form two sub-genera or genera. But as the original species (I) differed largely from (A), standing nearly at the extreme end of the original genus, the six descendants from (I) will, owing to inheritance alone, differ considerably from the eight descendants from (A); the two groups, moreover, are supposed to have gone on diverging in different directions. The intermediate species, also (and this is a very important consideration), which connected the original species (A) and (I), have all become, excepting (F), extinct, and have left no descendants. Hence the six new species descended from (I), and the eight descendants from (A), will have to be ranked as very distinct genera, or even as distinct sub-families.

Thus it is, as I believe, that two or more genera are produced by descent with modification from two or more species of the same genus. And the two or more parent species are supposed to be descended from some one species of an earlier genus. In our diagram, this is indicated by the broken lines, beneath the capital letters, converging in sub-branches downwards towards a single point; this point represents a species, the supposed progenitor of our several new sub-genera and genera.

We have seen that in each country it is the species belonging to the larger genera which oftenest present varieties or incipient species. This, indeed, might have been expected; for, as natural selection acts through one form having some advantage over other forms in the struggle for existence, it will chiefly act on those which already have some advantage; and the largeness of any group shows that its species have inherited from a common ancestor some advantage in common. Hence, the struggle for the production of new and modified descendants will mainly lie between

the larger groups which are all trying to increase in number. One large group will slowly conquer another large group, reduce its numbers, and thus lessen its chance of further variation and improvement. Within the same large group, the later and more highly perfected sub-groups, from branching out and seizing on many new places in the polity of Nature, will constantly tend to supplant and destroy the earlier and less improved sub-groups. Small and broken groups and sub-groups will finally disappear. Looking to the future, we can predict that the groups of organic beings which are now large and triumphant, and which are least broken up, that is, which have as yet suffered least extinction, will, for a long period, continue to increase. But which groups will ultimately prevail, no man can predict; for we know that many groups formerly most extensively developed have now become extinct. Looking still more remotely to the future, we may predict that, owing to the continued and steady increase of the larger groups, a multitude of smaller groups will become utterly extinct, and leave no modified descendants; and consequently that, of the species living at any one period, extremely few will transmit descendants to a remote futurity.

On the Degree to which Organisation tends to advance

Natural Selection acts exclusively by the preservation and accumulation of variations, which are beneficial under the organic and inorganic conditions to which each creature is exposed at all periods of life. The ultimate result is that each creature tends to become more and more improved in relation to its conditions. This improvement inevitably leads to the gradual advancement of the organisation of the greater number of living beings throughout the world. But here we enter on a very intricate subject, for naturalists have not defined to each other's satisfaction what is meant by an advance in organisation. Amongst the vertebrata the degree of intellect and an approach in structure to man clearly come into play. It might be thought that the amount of change which the various parts and organs pass through in their development from the embryo to maturity would suffice as a standard of comparison; but there are cases, as with certain parasitic crustaceans, in which several parts of the structure become less perfect, so that the mature animal cannot be called higher than its larva. Von Baer's standard seems the most widely applicable and the best, namely, the amount of differentiation of the parts of the same organic being, in the adult state as I should be inclined to add, and their specialisation for different functions; or, as Milne Edwards would express it, the completeness of the division of physiological labour.

But it may be objected that if all organic beings thus tend to rise in the scale, how is it that throughout the world a multitude of the lowest forms still exist; and how is it that in each great class some forms are far more highly developed than others? Why have not the more highly developed forms everywhere supplanted and exterminated the lower? Lamarck,

who believed in an innate and inevitable tendency towards perfection in all organic beings, seems to have felt this difficulty so strongly that he was led to suppose that new and simple forms are continually being produced by spontaneous generation. Science has not as yet proved the truth of this belief, whatever the future may reveal. On our theory the continued existence of lowly organisms offers no difficulty; for natural selection, or the survival of the fittest, does not necessarily include progressive development—it only takes advantage of such variations as arise and are beneficial to each creature under its complex relations of life. And it may be asked what advantage, as far as we can see, would it be to an infusorian animalcule—to an intestinal worm—or even to an earthworm, to be highly organised. If it were no advantage, these forms would be left, by natural selection, unimproved or but little improved, and might remain for indefinite ages in their present lowly condition. And geology tells us that some of the lowest forms, as the infusoria and rhizopods, have remained for an enormous period in nearly their present state.

Looking to the first dawn of life, when all organic beings, as we may believe, presented the simplest structure, how, it has been asked, could the first steps in the advancement or differentiation of parts have arisen? Mr. Herbert Spencer would probably answer that, as soon as simple unicellular organism came by growth or division to be compounded of several cells, or became attached to any supporting surface, his law "that homologous units of any order become differentiated in proportion as their relations to incident forces become different" would come into action. But as we have no facts to guide us, speculation on the subject is almost useless. It is, however, an error to suppose that there would be no struggle for existence, and, consequently, no natural selection, until many forms had been produced: variations in a single species inhabiting an isolated station might be beneficial, and thus the whole mass of individuals might be modified, or two distinct forms might arise. But, as I remarked towards the close of the Introduction, no one ought to feel surprise at much remaining as yet unexplained on the origin of species, if we make due allowance for our profound ignorance on the mutual relations of the inhabitants of the world at the present time, and still more so during past ages.

The affinities of all the beings of the same class have sometimes been represented by a great tree. I believe this simile largely speaks the truth. The green and budding twigs may represent existing species; and those produced during former years may represent the long succession of extinct species. At each period of growth all the growing twigs have tried to branch out on all sides, and to overtop and kill the surrounding twigs and branches, in the same manner as species and groups of species have at all times overmastered other species in the great battle for life. The limbs divided into great branches, and these into lesser and lesser branches, were themselves once, when the tree was young, budding twigs, and this connection of the former and present buds by ramifying branches may well represent the classification of all extinct and living species in

groups subordinate to groups. Of the many twigs which flourished when the tree was a mere bush, only two or three, now grown into great branches, yet survive and bear the other branches; so with the species which lived during long-past geological periods, very few have left living and modified descendants. From the first growth of the tree, many a limb and branch has decayed and dropped off; and these fallen branches of various sizes may represent those whole orders, families, and genera which have now no living representatives, and which are known to us only in a fossil state. As we here and there see a thin straggling branch springing from a fork low down in a tree, and which by some chance has been favoured and is still alive on its summit, so we occasionally see an animal like the Ornithorhynchus or Lepidosiren, which in some small degree connects by its affinities two large branches of life, and which has apparently been saved from fatal competition by having inhabited a protected station. As buds give rise by growth to fresh buds, and these, if vigorous, branch out and overtop on all sides many a feebler branch, so by generation I believe it has been with the great Tree of Life, which fills with its dead and broken branches the crust of the earth, and covers the surface with its ever-branching and beautiful ramifications.

V. LAWS OF VARIATION

I HAVE hitherto sometimes spoken as if the variations—so common and multiform with organic beings under domestication, and in a lesser degree with those under nature—were due to chance. This, of course, is a wholly incorrect expression, but it serves to acknowledge plainly our ignorance of the cause of each particular variation. Some authors believe it to be as much the function of the reproductive system to produce individual differences, or slight deviations of structure, as to make the child like its parents. But the fact of variations and monstrosities occurring much more frequently under domestication than under nature, and the greater variability of species having wider ranges than of those with restricted ranges, leads to the conclusion that variability is generally related to the conditions of life to which each species has been exposed during several successive generations. In the first chapter I attempted to show that changed conditions act in two ways, directly on the whole organisation or on certain parts alone, and indirectly through the reproductive system. In all cases there are two factors, the nature of the organism, which is much the most important of the two, and the nature of the conditions. The direct action of changed conditions leads to definite or indefinite results. In the latter case the organisation seems to become plastic, and we have much fluctuating variability. In the former case the nature of the organism is such that it yields readily, when subjected to certain conditions, and all, or nearly all, the individuals become modified in the same way.

It is very difficult to decide how far changed conditions, such as of climate, food, &c., have acted in a definite manner. There is reason to be-

lieve that in the course of time the effects have been greater than can be proved by clear evidence. But we may safely conclude that the innumerable complex coadaptations of structure, which we see throughout nature between various organic beings, cannot be attributed simply to such action.

When a variation is of the slightest use to any being, we cannot tell how much to attribute to the accumulative action of natural selection and how much to the definite action of the conditions of life. Thus, it is well known to furriers that animals of the same species have thicker and better fur the further north they live; but who can tell how much of this difference may be due to the warmest-clad individuals having been favoured and preserved during many generations, and how much to the action of the severe climate? for it would appear that climate has some direct action on the hair of our domestic quadrupeds.

In one sense the conditions of life may be said, not only to cause variability, either directly or indirectly, but likewise to include natural selection, for the conditions determine whether this or that variety shall survive. But when man is the selecting agent, we clearly see that the two elements of change are distinct; variability is in some manner excited, but it is the will of man which accumulates the variations in certain directions; and it is this latter agency which answers to the survival of the fittest under nature.

Effects of the increased Use and Disuse of Parts, as controlled by Natural Selection

From the facts alluded to in the first chapter, I think there can be no doubt that use in our domestic animals has strengthened and enlarged certain parts, and disuse diminished them; and that such modifications are inherited. Under free nature, we have no standard of comparison, by which to judge of the effects of long-continued use or disuse, for we know not the parent forms; but many animals possess structures which can be best explained by the effects of disuse.

It is well known that several animals, belonging to the most different classes, which inhabit the caves of Carniola and of Kentucky, are blind. In some of the crabs the footstalk for the eye remains, though the eye is gone;—the stand for the telescope is there, though the telescope with its glasses has been lost. As it is difficult to imagine that eyes, though useless, could be in any way injurious to animals living in darkness, their loss may be attributed to disuse. In one of the blind animals, namely, the cave rat (Noetoma), two of which were captured by Professor Silliman at above half a mile distance from the mouth of the cave, and therefore not in the profoundest depths, the eyes were lustrous and of large size; and these animals, as I am informed by Professor Silliman, after having been exposed for about a month to a graduated light, acquired a dim perception of objects.

It is difficult to imagine conditions of life more similar than deep limestone caverns under a nearly similar climate; so that, in accordance with the old view of the blind animals having been separately created for the American and European caverns, very close similarity in their organisation and affinities might have been expected. This is certainly not the case if we look at the two whole faunas. On my view we must suppose that American animals, having in most cases ordinary powers of vision, slowly migrated by successive generations from the outer world into the deeper and deeper recesses of the Kentucky caves, as did European animals into the caves of Europe. By the time that an animal had reached, after numberless generations, the deepest recesses, disuse will on this view have more or less perfectly obliterated its eyes, and natural selection will often have effected other changes, such as an increase in the length of the antennæ or palpi, as a compensation for blindness. Notwithstanding such modifications, we might expect still to see, in the cave animals of America, affinities to the other inhabitants of that continent, and in those of Europe to the inhabitants of the European continent. And this is the case with some of the American cave animals, as I hear from Professor Dana; and some of the European cave insects are very closely allied to those of the surrounding country. It would be difficult to give any rational explanation of the affinities of the blind cave animals to the other inhabitants of the two continents on the ordinary view of their independent creation.

Correlated Variation

I mean by this expression that the whole organisation is so tied together during its growth and development that when slight variations in any one part occur, and are accumulated through natural selection, other parts become modified. This is a very important subject, most imperfectly understood, and no doubt wholly different classes of facts may be here easily confounded together. We shall presently see that simple inheritance often gives the false appearance of correlation. One of the most obvious real cases is that variations of structure arising in the young or larvæ naturally tend to affect the structure of the mature animal. The several parts of the body which are homologous, and which, at an early embryonic period, are identical in structure, and which are necessarily exposed to similar conditions, seem eminently liable to vary in a like manner: we see this in the right and left sides of the body varying in the same manner; in the front and hind legs, and even in the jaws and limbs, varying together, for the lower jaw is believed by some anatomists to be homologous with the limbs. These tendencies, I do not doubt, may be mastered more or less completely by natural selection; thus a family of stags once existed with an antler only on one side; and if this had been of any great use to the breed, it might probably have been rendered permanent by selection.

We may often falsely attribute to correlated variation structures

which are common to whole groups of species, and which in truth are simply due to inheritance; for an ancient progenitor may have acquired through natural selection some one modification in structure, and, after thousands of generations, some other and independent modification; and these two modifications, having been transmitted to a whole group of descendants with diverse habits, would naturally be thought to be in some necessary manner correlated. Some other correlations are apparently due to the manner in which natural selection can alone act. For instance, Alphonse de Candolle has remarked that winged seeds are never found in fruits which do not open; I should explain this rule by the impossibility of seeds gradually becoming winged through natural selection, unless the capsules were open; for in this case alone could the seeds, which were a little better adapted to be wafted by the wind, gain an advantage over others less well fitted for wide dispersal.

When we see any part or organ developed in a remarkable degree or manner in a species, the fair presumption is that it is of high importance to that species: nevertheless it is in this case eminently liable to variation. Why should this be so? On the view that each species has been independently created, with all its parts as we now see them, I can see no explanation. But on the view that groups of species are descended from some other species, and have been modified through natural selection, I think we can obtain some light.

When a part has been developed in an extraordinary manner in any one species, compared with the other species of the same genus, we may conclude that this part has undergone an extraordinary amount of modification since the period when the several species branched off from the common progenitor of the genus. This period will seldom be remote in any extreme degree, as species rarely endure for more than one geological period. An extraordinary amount of modification implies an unusually large and long-continued amount of variability, which has continually been accumulated by natural selection for the benefit of the species. But as the variability of the extraordinarily developed part or organ has been so great and long-continued within a period not excessively remote, we might, as a general rule, still expect to find more variability in such parts than in other parts of the organisation which have remained for a much longer period nearly constant. And this, I am convinced, is the case. That the struggle between natural selection, on the one hand, and the tendency to reversion and variability, on the other hand, will in the course of time cease; and that the most abnormally developed organs may be made constant, I see no reason to doubt. Hence, when an organ, however abnormal it may be, has been transmitted in approximately the same condition to many modified descendants, as in the case of the wing of the bat, it must have existed, according to our theory, for an immense period in nearly the same state; and thus it has come not to be more variable than any other structure.

Specific Characters more Variable than Generic Characters

It is notorious that specific characters are more variable than generic. To explain by a simple example what is meant: if in a large genus of plants some species had blue flowers and some had red, the colour would be only a specific character, and no one would be surprised at one of the blue species varying into red, or conversely; but if all the species had blue flowers, the colour would become a generic character, and its variation would be a more unusual circumstance.

On the ordinary view of each species having been independently created, why should that part of the structure which differs from the same part in other independently created species of the same genus be more variable than those parts which are closely alike in the several species? I do not see that any explanation can be given. But on the view that species are only strongly marked and fixed varieties, we might expect often to find them still continuing to vary in those parts of their structure which have varied within a moderately recent period, and which have thus come to differ. Or to state the case in another manner:—the points in which all the species of a genus resemble each other, and in which they differ from allied genera, are called generic characters; and these characters may be attributed to inheritance from a common progenitor, for it can rarely have happened that natural selection will have modified several distinct species, fitted to more or less widely different habits, in exactly the same manner: and as these so-called generic characters have been inherited from before the period when the several species first branched off from their common progenitor, and subsequently have not varied or come to differ in any degree, or only in a slight degree, it is not probable that they should vary at the present day. On the other hand, the points in which species differ from other species of the same genus are called specific characters; and as these specific characters have varied and come to differ since the period when the species branched off from a common progenitor, it is probable that they should still often be in some degree variable—at least more variable than those parts of the organisation which have for a very long period remained constant.

Distinct Species present analogous Variations, so that a Variety of one Species often assumes a Character proper to an allied Species, or reverts to some of the Characters of an early Progenitor.—These propositions will be most readily understood by looking to our domestic races. The most distinct breeds of the pigeon, in countries widely apart, present sub-varieties with reversed feathers on the head, and with feathers on the feet—characters not possessed by the aboriginal rock pigeon; these then are analogous variations in two or more distinct races. The frequent presence of fourteen or even sixteen tail feathers in the pouter may be considered as a variation representing the normal structure of another race, the fantail. I presume that no one will doubt that all such analogous varia-

tions are due to the several races of the pigeon having inherited from a common parent the same constitution and tendency to variation, when acted on by similar unknown influences.

With pigeons, however, we have another case, namely, the occasional appearance in all the breeds of slaty-blue birds with two black bars on the wings, white loins, a bar at the end of the tail, with the outer feathers externally edged near their basis with white. As all these marks are characteristic of the parent rock pigeon, I presume that no one will doubt that this is a case of reversion, and not of a new yet analogous variation appearing in the several breeds. We may, I think, confidently come to this conclusion, because, as we have seen, these coloured marks are eminently liable to appear in the crossed offspring of two distinct and differently coloured breeds; and in this case there is nothing in the external conditions of life to cause the reappearance of the slaty-blue, with the several marks, beyond the influence of the mere act of crossing on the laws of inheritance.

I will give one curious and complex case, not indeed as affecting any important character, but from occurring in several species of the same genus, partly under domestication and partly under nature. It is a case almost certainly of reversion. The ass sometimes has very distinct transverse bars on its legs, like those on the legs of the zebra: it has been asserted that these are plainest in the foal, and, from inquiries which I have made, I believe this to be true. The stripe on the shoulder is sometimes double, and is very variable in length and outline. A white ass, but *not* an albino, has been described without either spinal or shoulder stripe: and these stripes are sometimes very obscure, or actually quite lost, in dark-coloured asses. The koulan of Pallas is said to have been seen with a double shoulder stripe. Mr. Blyth has seen a specimen of the hemionus with a distinct shoulder stripe, though it properly has none; and I have been informed by Colonel Poole that the foals of this species are generally striped on the legs, and faintly on the shoulder. The quagga, though so plainly barred like a zebra over the body, is without bars on the legs; but Dr. Gray has figured one specimen with very distinct zebra-like bars on the hocks.

With respect to the horse, I have collected cases in England of the spinal stripe in horses of the most distinct breeds, and of *all* colours: transverse bars on the legs are not rare in duns, mouse duns, and in one instance in a chestnut: a faint shoulder stripe may sometimes be seen in duns, and I have seen a trace in a bay horse. My son made a careful examination and sketch for me of a dun Belgian cart horse with a double stripe on each shoulder and with leg stripes; I have myself seen a dun Devonshire pony, and a small dun Welsh pony has been carefully described to me, both with *three* parallel stripes on each shoulder.

Now let us turn to the effects of crossing the several species of the horse genus. Rollin asserts that the common mule from the ass and horse is particularly apt to have bars on its legs; according to Mr. Gosse, in certain parts of the United States about nine out of ten mules have

striped legs. I once saw a mule with its legs so much striped that anyone might have thought that it was a hybrid zebra; and Mr. W. C. Martin, in his excellent treatise on the horse, has given a figure of a similar mule. In four coloured drawings, which I have seen, of hybrids between the ass and zebra, the legs were much more plainly barred than the rest of the body; and in one of them there was a double shoulder stripe. In Lord Morton's famous hybrid, from a chestnut mare and male quagga, the hybrid and even the pure offspring subsequently produced from the same mare by a black Arabian sire were much more plainly barred across the legs than is even the pure quagga. Lastly, and this is another most remarkable case, a hybrid has been figured by Dr. Gray (and he informs me that he knows of a second case) from the ass and the hemionus; and this hybrid, though the ass only occasionally has stripes on its legs and the hemionus has none and has not even a shoulder stripe, nevertheless had all four legs barred, and had three short shoulder stripes, like those on the dun Devonshire and Welsh ponies, and even had some zebra-like stripes on the sides of its face.

What now are we to say to these several facts? We see several distinct species of the horse genus becoming, by simple variation, striped on the legs like a zebra, or striped on the shoulders like an ass. In the horse we see this tendency strong whenever a dun tint appears—a tint which approaches to that of the general colouring of the other species of the genus. The appearance of the stripes is not accompanied by any change of form or by any other new character. We see this tendency to become striped most strongly displayed in hybrids from between several of the most distinct species. Now observe the case of the several breeds of pigeons: they are descended from a pigeon (including two or three subspecies or geographical races) of a bluish colour, with certain bars and other marks; and when any breed assumes by simple variation a bluish tint, these bars and other marks invariably reappear; but without any other change of form or character. When the oldest and truest breeds of various colours are crossed, we see a strong tendency for the blue tint and bars and marks to reappear in the mongrels. I have stated that the most probable hypothesis to account for the reappearance of very ancient characters is—that there is a *tendency* in the young of each successive generation to produce the long-lost character, and that this tendency, from unknown causes, sometimes prevails. And we have just seen that in several species of the horse genus the stripes are either plainer or appear more commonly in the young than in the old. Call the breeds of pigeons, some of which have bred true for centuries, species; and how exactly parallel is the case with that of the species of the horse genus! For myself, I venture confidently to look back thousands on thousands of generations, and I see an animal striped like a zebra, but perhaps otherwise very differently constructed, the common parent of our domestic horse (whether or not it be descended from one or more wild stocks), of the ass, the hemionus, quagga, and zebra.

VI. DIFFICULTIES OF THE THEORY

LONG BEFORE the reader has arrived at this part of my work, a crowd of difficulties will have occurred to him. Some of them are so serious that to this day I can hardly reflect on them without being in some degree staggered; but, to the best of my judgment, the greater number are only apparent, and those that are real are not, I think, fatal to the theory.

These difficulties and objections may be classed under the following heads:—First, why, if species have descended from other species by fine gradations, do we not everywhere see innumerable transitional forms? Why is not all nature in confusion, instead of the species being, as we see them, well defined?

Secondly, is it possible that an animal having, for instance, the structure and habits of a bat could have been formed by the modification of some other animal with widely different habits and structure? Can we believe that natural selection could produce, on the one hand, an organ of trifling importance, such as the tail of a giraffe, which serves as a fly flapper, and, on the other hand, an organ so wonderful as the eye?

Thirdly, can instincts be acquired and modified through natural selection?

On the Absence or Rarity of Transitional Varieties.—As natural selection acts solely by the preservation of profitable modifications, each new form will tend in a fully stocked country to take the place of, and finally to exterminate, its own less improved parent form and other less favoured forms with which it comes into competition. Thus extinction and natural selection go hand in hand. Hence, if we look at each species as descended from some unknown form, both the parent and all the transitional varieties will generally have been exterminated by the very process of the formation and perfection of the new form.

But it may be urged that when several closely allied species inhabit the same territory, we surely ought to find at the present time many transitional forms. Let us take a simple case: in travelling from north to south over a continent, we generally meet at successive intervals with closely allied or representative species, evidently filling nearly the same place in the natural economy of the land. These representative species often meet and interlock; and as the one becomes rarer and rarer, the other becomes more and more frequent, till the one replaces the other. But if we compare these species where they intermingle, they are generally as absolutely distinct from each other in every detail of structure as are specimens taken from the metropolis inhabited by each. By my theory these allied species are descended from a common parent; and during the process of modification, each has become adapted to the conditions of life of its own region, and has supplanted and exterminated its original parent form and all the transitional varieties between its past and present states. Hence we ought not to expect at the present time to meet with numerous transi-

tional varieties in each region, though they must have existed there, and may be embedded there in a fossil condition. But in the intermediate region, having intermediate conditions of life, why do we not now find closely linking intermediate varieties? This difficulty for a long time quite confounded me. But I think it can be in large part explained.

As allied or representative species, when inhabiting a continuous area, are generally distributed in such a manner that each has a wide range, with a comparatively narrow neutral territory between them, in which they become rather suddenly rarer and rarer; then, as varieties do not essentially differ from species, the same rule will probably apply to both; and if we take a varying species inhabiting a very large area, we shall have to adapt two varieties to two large areas, and a third variety to a narrow intermediate zone. The intermediate variety, consequently, will exist in lesser numbers from inhabiting a narrow and lesser area; and practically, as far as I can make out, this rule holds good with varieties in a state of nature. Now, if varieties linking two other varieties together generally have existed in lesser numbers than the forms which they connect, then we can understand why intermediate varieties should not endure for very long periods:—why, as a general rule, they should be exterminated and disappear sooner than the forms which they originally linked together.

For any form existing in lesser numbers would, as already remarked, run a greater chance of being exterminated than one existing in large numbers; and in this particular case the intermediate form would be eminently liable to the inroads of closely allied forms existing on both sides of it. Hence, the more common forms, in the race for life, will tend to beat and supplant the less common forms, for these will be more slowly modified and improved. It is the same principle which, as I believe, accounts for the common species in each country, as shown in the second chapter, presenting on an average a greater number of well-marked varieties than do the rarer species. I may illustrate what I mean by supposing three varieties of sheep to be kept, one adapted to an extensive mountainous region; a second to a comparatively narrow, hilly tract; and a third to the wide plains at the base; and that the inhabitants are all trying with equal steadiness and skill to improve their stocks by selection; the chances in this case will be strongly in favour of the great holders on the mountains or on the plains improving their breeds more quickly than the small holders on the intermediate narrow, hilly tract; and consequently the improved mountain or plain breed will soon take the place of the less improved hill breed; and thus the two breeds, which originally existed in greater numbers, will come into close contact with each other, without the interposition of the supplanted, intermediate hill variety.

To sum up, I believe that species come to be tolerably well-defined objects, and do not at any one period present an inextricable chaos of varying and intermediate links; first, because new varieties are very slowly formed, for variation is a slow process, and natural selection can do nothing until favourable individual differences or variations occur, and

until a place in the natural polity of the country can be better filled by some modification of some one or more of its inhabitants. And such new places will depend on slow changes of climate, or on the occasional immigration of new inhabitants, and, probably, in a still more important degree, on some of the old inhabitants becoming slowly modified, with the new forms thus produced, and the old ones acting and reacting on each other. So that, in any one region and at any one time, we ought to see only a few species presenting slight modifications of structure in some degree permanent; and this assuredly we do see.

Secondly, areas now continuous must often have existed within the recent period as isolated portions, in which many forms, more especially amongst the classes which unite for each birth and wander much, may have separately been rendered sufficiently distinct to rank as representative species. In this case, intermediate varieties between the several representative species and their common parent must formerly have existed within each isolated portion of the land, but these links during the process of natural selection will have been supplanted and exterminated, so that they will no longer be found in a living state.

Thirdly, when two or more varieties have been formed in different portions of a strictly continuous area, intermediate varieties will, it is probable, at first have been formed in the intermediate zones, but they will generally have had a short duration. For these intermediate varieties will, from reasons already assigned (namely from what we know of the actual distribution of closely allied or representative species, and likewise of acknowledged varieties), exist in the intermediate zones in lesser numbers than the varieties which they tend to connect. From this cause alone the intermediate varieties will be liable to accidental extermination; and during the process of further modification through natural selection, they will almost certainly be beaten and supplanted by the forms which they connect; for these, from existing in greater numbers, will, in the aggregate, present more varieties, and thus be further improved through natural selection and gain further advantages.

Lastly, looking not to any one time, but to all time, if my theory be true, numberless intermediate varieties, linking closely together all the species of the same group, must assuredly have existed; but the very process of natural selection constantly tends, as has been so often remarked, to exterminate the parent forms and the intermediate links. Consequently evidence of their former existence could be found only amongst fossil remains, which are preserved, as we shall attempt to show in a future chapter, in an extremely imperfect and intermittent record.

On the Origin and Transitions of Organic Beings with peculiar Habits and Structure.—It has been asked by the opponents of such views as I hold how, for instance, could a land carnivorous animal have been converted into one with aquatic habits; for how could the animal in its transitional state have subsisted? It would be easy to show that there now exist carnivorous animals presenting close intermediate grades from strictly terrestrial to aquatic habits; and as each exists by a struggle for

life, it is clear that each must be well adapted to its place in nature. Look at the Mustela vison of North America, which has webbed feet, and which resembles an otter in its fur, short legs, and form of tail. During the summer this animal dives for and preys on fish, but during the long winter it leaves the frozen waters, and preys, like other polecats, on mice and land animals.

Look at the family of squirrels; here we have the finest gradation from animals with their tails only slightly flattened, and from others, as Sir J. Richardson has remarked, with the posterior part of their bodies rather wide and with the skin on their flanks rather full, to the so-called flying squirrels; and flying squirrels have their limbs and even the base of the tail united by a broad expanse of skin, which serves as a parachute and allows them to glide through the air to an astonishing distance from tree to tree. We cannot doubt that each structure is of use to each kind of squirrel in its own country, by enabling it to escape birds or beasts of prey, to collect food more quickly, or, as there is reason to believe, to lessen the danger from occasional falls. But it does not follow from this fact that the structure of each squirrel is the best that it is possible to conceive under all possible conditions. Let the climate and vegetation change, let other competing rodents or new beasts of prey immigrate, or old ones become modified, and all analogy would lead us to believe that some at least of the squirrels would decrease in numbers or become exterminated, unless they also become modified and improved in structure in a corresponding manner. Therefore, I can see no difficulty, more especially under changing conditions of life, in the continued preservation of individuals with fuller and fuller flank membranes, each modification being useful, each being propagated, until, by the accumulated effects of this process of natural selection, a perfect so-called flying squirrel was produced.

Now look at the Galeopithecus or so-called flying lemur, which formerly was ranked amongst bats, but is now believed to belong to the Insectivora. An extremely wide flank membrane stretches from the corners of the jaw to the tail, and includes the limbs with the elongated fingers. This flank membrane is furnished with an extensor muscle. Although no graduated links of structure, fitted for gliding through the air, now connect the Galeopithecus with the other Insectivora, yet there is no difficulty in supposing that such links formerly existed, and that each was developed in the same manner as with the less perfectly gliding squirrels; each grade of structure having been useful to its possessor. Nor can I see any insuperable difficulty in further believing that the membrane connected fingers and forearm of the Galeopithecus might have been greatly lengthened by natural selection; and this, as far as the organs of flight are concerned, would have converted the animal into a bat. In certain bats in which the wing membrane extends from the top of the shoulder to the tail and includes the hind legs, we perhaps see traces of an apparatus originally fitted for gliding through the air rather than for flight.

It is, however, difficult to decide, and immaterial for us, whether

habits generally change first and structure afterwards; or whether slight modifications of structure lead to changed habits; both probably often occurring almost simultaneously. Of cases of changed habits it will suffice merely to allude to that of the many British insects which now feed on exotic plants, or exclusively on artificial substances. Of diversified habits innumerable instances could be given: I have often watched a tyrant fly-catcher (Saurophagus sulphuratus) in South America, hovering over one spot and then proceeding to another, like a kestrel, and at other times standing stationary on the margin of water, and then dashing into it like a kingfisher at a fish. In our own country the larger titmouse (Parus major) may be seen climbing branches, almost like a creeper; it sometimes, like a shrike, kills small birds by blows on the head; and I have many times seen and heard it hammering the seeds of the yew on a branch, and thus breaking them like a nuthatch. In North America the black bear was seen by Hearne swimming for hours with widely open mouth, thus catching, almost like a whale, insects in the water.

He who believes in separate and innumerable acts of creation may say that in these cases it has pleased the Creator to cause a being of one type to take the place of one belonging to another type; but this seems to me only restating the fact in dignified language. He who believes in the struggle for existence and in the principle of natural selection will acknowledge that every organic being is constantly endeavouring to increase in numbers; and that if any one being varies ever so little, either in habits or structure, and thus gains an advantage over some other inhabitant of the same country, it will seize on the place of that inhabitant, however different that may be from its own place. Hence it will cause him no surprise that there should be geese and frigate birds with webbed feet living on the dry land and rarely alighting on the water, that there should be long-toed corn crakes living in meadows instead of in swamps; that there should be woodpeckers where hardly a tree grows; that there should be diving thrushes and diving Hymenoptera, and petrels with the habits of auks.

Organs of extreme Perfection and Complication

To suppose that the eye, with all its inimitable contrivances for adjusting the focus to different distances, for admitting different amounts of light, and for the correction of spherical and chromatic aberration, could have been formed by natural selection seems, I freely confess, absurd in the highest degree. When it was first said that the sun stood still and the world turned round, the common sense of mankind declared the doctrine false; but the old saying of *Vox populi, vox Dei,* as every philosopher knows, cannot be trusted in science. Reason tells me that if numerous gradations from a simple and imperfect eye to one complex and perfect can be shown to exist, each grade being useful to its possessor, as is certainly the case; if, further, the eye ever varies and the variations be inherited, as is likewise certainly the case; and if such variations should be

useful to any animal under changing conditions of life, then the difficulty of believing that a perfect and complex eye could be formed by natural selection, though insuperable by our imagination, should not be considered as subversive of the theory.

In searching for the gradations through which an organ in any species has been perfected, we ought to look exclusively to its lineal progenitors; but this is scarcely ever possible, and we are forced to look to other species and genera of the same group, that is to the collateral descendants from the same parent form, in order to see what gradations are possible, and for the chance of some gradations having been transmitted in an unaltered or little altered condition. But the state of the same organ in distinct classes may incidentally throw light on the steps by which it has been perfected.

The simplest organ which can be called an eye consists of an optic nerve, surrounded by pigment cells and covered by translucent skin, but without any lens or other refractive body. We may, however, according to M. Jourdain, descend even a step lower and find aggregates of pigment cells, apparently serving as organs of vision, without any nerves, and resting merely on sarcodic tissue. Eyes of the above simple nature are not capable of distinct vision, and serve only to distinguish light from darkness. In certain star fishes, small depressions in the layer of pigment which surrounds the nerve are filled, as described by the author just quoted, with transparent gelatinous matter, projecting with a convex surface, like the cornea in the higher animals. He suggests that this serves not to form an image, but only to concentrate the luminous rays and render their perception more easy. In this concentration of the rays we gain the first and by far the most important step towards the formation of a true, picture-forming eye; for we have only to place the naked extremity of the optic nerve, which in some of the lower animals lies deeply buried in the body, and in some near the surface, at the right distance from the concentrating apparatus, and an image will be formed on it.

Within the highest division of the animal kingdom, namely, the Vertebrata, we can start from an eye so simple that it consists, as in the lancelet, of a little sack of transparent skin, furnished with a nerve and lined with pigment, but destitute of any other apparatus. In fishes and reptiles, as Owen has remarked, "the range of gradations of dioptric structures is very great." It is a significant fact that even in man, according to the high authority of Virchow, the beautiful crystalline lens is formed in the embryo by an accumulation of epidermic cells, lying in a sack-like fold of the skin; and the vitreous body is formed from embryonic subcutaneous tissue. To arrive, however, at a just conclusion regarding the formation of the eye, with all its marvellous yet not absolutely perfect characters, it is indispensable that the reason should conquer the imagination; but I have felt the difficulty far too keenly to be surprised at others hesitating to extend the principle of natural selection to so startling a length.

It is scarcely possible to avoid comparing the eye with a telescope. We

know that this instrument has been perfected by the long-continued efforts of the highest human intellects; and we naturally infer that the eye has been formed by a somewhat analogous process. But may not this inference be presumptuous? Have we any right to assume that the Creator works by intellectual powers like those of man? If we must compare the eye to an optical instrument, we ought in imagination to take a thick layer of transparent tissue, with spaces filled with fluid, and with a nerve sensitive to light beneath, and then suppose every part of this layer to be continually changing slowly in density, so as to separate into layers of different densities and thicknesses, placed at different distances from each other, and with the surfaces of each layer slowly changing in form. Further we must suppose that there is a power, represented by natural selection or the survival of the fittest, always intently watching each slight alteration in the transparent layers; and carefully preserving each which, under varied circumstances, in any way or in any degree, tends to produce a distincter image. We must suppose each new state of the instrument to be multiplied by the million; each to be preserved until a better one is produced, and then the old ones to be all destroyed. In living bodies, variation will cause the slight alterations, generation will multiply them almost infinitely, and natural selection will pick out with unerring skill each improvement. Let this process go on for millions of years; and during each year on millions of individuals of many kinds; and may we not believe that a living optical instrument might thus be formed as superior to one of glass, as the works of the Creator are to those of man?

Modes of Transition

If it could be demonstrated that any complex organ existed which could not possibly have been formed by numerous, successive, slight modifications, my theory would absolutely break down. But I can find out no such case. No doubt many organs exist of which we do not know the transitional grades, more especially if we look to much-isolated species, round which, according to the theory, there has been much extinction. Or again, if we take an organ common to all the members of a class, for in this latter case the organ must have been originally formed at a remote period, since which all the many members of the class have been developed; and in order to discover the early transitional grades through which the organ has passed, we should have to look to very ancient ancestral forms, long since become extinct.

We should be extremely cautious in concluding that an organ could not have been formed by transitional gradations of some kind. Numerous cases could be given amongst the lower animals of the same organ performing at the same time wholly distinct functions; thus in the larva of the dragonfly and in the fish Cobites the alimentary canal respires, digests, and excretes. In the Hydra, the animal may be turned inside out, and the exterior surface will then digest and the stomach respire. In such cases

natural selection might specialise, if any advantage were thus gained, the whole or part of an organ, which had previously performed two functions, for one function alone, and thus by insensible steps greatly change its nature.

The illustration of the swim bladder in fishes is a good one, because it shows us clearly the highly important fact that an organ originally constructed for one purpose, namely, flotation, may be converted into one for a widely different purpose, namely, respiration. The swim bladder has, also, been worked in as an accessory to the auditory organs of certain fishes. All physiologists admit that the swim bladder is homologous, or "ideally similar" in position and structure with the lungs of the higher vertebrate animals: hence there is no reason to doubt that the swim bladder has actually been converted into lungs, or an organ used exclusively for respiration.

According to this view it may be inferred that all vertebrate animals with true lungs are descended by ordinary generation from an ancient and unknown prototype, which was furnished with a floating apparatus or swim bladder. We can thus, as I infer from Owen's interesting description of these parts, understand the strange fact that every particle of food and drink which we swallow has to pass over the orifice of the trachea, with some risk of falling into the lungs, notwithstanding the beautiful contrivance by which the glottis is closed. In the higher Vertebrate the branchiæ have wholly disappeared—but in the embryo the slits on the sides of the neck and the loop-like course of the arteries still mark their former position.

It is a common rule throughout nature that the same end should be gained, even sometimes in the case of closely related beings, by the most diversified means. How differently constructed is the feathered wing of a bird and the membrane-covered wing of a bat; and still more so the four wings of a butterfly, the two wings of a fly, and the two wings with the elytra of a beetle. Bivalve shells are made to open and shut, but on what a number of patterns is the hinge constructed—from the long row of neatly interlocking teeth in a Nucula to the simple ligament of a Mussel!

With plants having separated sexes, and with those in which, though hermaphrodites, the pollen does not spontaneously fall on the stigma, some aid is necessary for their fertilisation. With several kinds this is effected by the pollen grains, which are light and incoherent, being blown by the wind through mere chance on to the stigma; and this is the simplest plan which can well be conceived. An almost equally simple, though very different, plan occurs in many plants in which a symmetrical flower secretes a few drops of nectar, and is consequently visited by insects; and these carry the pollen from the anthers to the stigma.

From this simple stage we may pass through an inexhaustible number of contrivances, all for the same purpose and effected in essentially the same manner, but entailing changes in every part of the flower. The nectar may be stored in variously shaped receptacles, with the stamens and pistils modified in many ways, sometimes forming trap-like contriv-

ances, and sometimes capable of neatly adapted movements through irritability or elasticity. From such structures we may advance till we come to such a case of extraordinary adaptation as that lately described by Dr. Crüger in the Coryanthes. This orchid has part of its labellum or lower lip hollowed out into a great bucket, into which drops of almost pure water continually fall from two secreting horns which stand above it; and when the bucket is half full, the water overflows by a spout on one side. The basal part of the labellum stands over the bucket, and is itself hollowed out into a sort of chamber with two lateral entrances; within this chamber there are curious fleshy ridges. The most ingenious man, if he had not witnessed what takes place, could never have imagined what purpose all these parts serve. But Dr. Crüger saw crowds of large humblebees visiting the gigantic flowers of this orchid, not in order to suck nectar, but to gnaw off the ridges within the chamber above the bucket; in doing this they frequently pushed each other into the bucket, and their wings being thus wetted they could not fly away, but were compelled to crawl out through the passage formed by the spout or overflow. Dr. Crüger saw a "continual procession" of bees thus crawling out of their involuntary bath. The passage is narrow, and is roofed over by the column, so that a bee, in forcing its way out, first rubs its back against the viscid stigma and then against the viscid glands of the pollen masses. The pollen masses are thus glued to the back of the bee which first happens to crawl out through the passage of a lately expanded flower, and are thus carried away. Dr. Crüger sent me a flower in spirits of wine, with a bee which he had killed before it had quite crawled out with a pollen mass still fastened to its back. When the bee, thus provided, flies to another flower, or to the same flower a second time, and is pushed by its comrades into the bucket and then crawls out by the passage, the pollen mass necessarily comes first into contact with the viscid stigma, and adheres to it, and the flower is fertilised. Now at last we see the full use of every part of the flower, of the water-secreting horns, of the bucket half full of water, which prevents the bees from flying away, and forces them to crawl out through the spout, and rub against the properly placed viscid pollen masses and the viscid stigma.

How, it may be asked, in the foregoing and in innumerable other instances, can we understand the graduated scale of complexity and the multifarious means for gaining the same end? The answer no doubt is, as already remarked, that when two forms vary, which already differ from each other in some slight degree, the variability will not be of the same exact nature, and consequently the results obtained through natural selection for the same general purpose will not be the same. We should also bear in mind that every highly developed organism has passed through many changes; and that each modified structure tends to be inherited, so that each modification will not readily be quite lost, but may be again and again further altered. Hence the structure of each part of each species, for whatever purpose it may serve, is the sum of many inherited changes, through which the species has passed during its successive adaptations to changed habits and conditions of life.

Finally then, although in many cases it is most difficult even to conjecture by what transitions organs have arrived at their present state; yet, considering how small the proportion of living and known forms is to the extinct and unknown, I have been astonished how rarely an organ can be named, towards which no transitional grade is known to lead. It certainly is true that new organs appearing as if created for some special purpose rarely or never appear in any being;—as indeed is shown by that old, but somewhat exaggerated, canon in natural history of "Natura non facit saltum." We meet with this admission in the writings of almost every experienced naturalist; or, as Milne Edwards has well expressed it, Nature is prodigal in variety, but niggard in innovation. Why, on the theory of Creation, should there be so much variety and so little real novelty? Why should all the parts and organs of many independent beings, each supposed to have been separately created for its proper place in nature, be so commonly linked together by graduated steps? Why should not Nature take a sudden leap from structure to structure? On the theory of natural selection, we can clearly understand why she should not; for natural selection acts only by taking advantage of slight successive variations; she can never take a great and sudden leap, but must advance by short and sure, though slow, steps.

Natural selection cannot possibly produce any modification in a species exclusively for the good of another species; though throughout nature one species incessantly takes advantage of, and profits by, the structures of others. But natural selection can and does often produce structures for the direct injury of other animals, as we see in the fang of the adder, and in the ovipositor of the ichneumon, by which its eggs are deposited in the living bodies of other insects. If it could be proved that any part of the structure of any one species had been formed for the exclusive good of another species, it would annihilate my theory, for such could not have been produced through natural selection. Although many statements may be found in works on natural history to this effect, I cannot find even one which seems to me of any weight. It is admitted that the rattlesnake has a poison fang for its own defence, and for the destruction of its prey; but some authors suppose that at the same time it is furnished with a rattle for its own injury, namely, to warn its prey. I would almost as soon believe that the cat curls the end of its tail when preparing to spring, in order to warn the doomed mouse. It is a much more probable view that the rattlesnake uses its rattle, the cobra expands its frill, and the puff adder swells whilst hissing so loudly and harshly, in order to alarm the many birds and beasts which are known to attack even the most venomous species.

Natural selection tends only to make each organic being as perfect as, or slightly more perfect than, the other inhabitants of the same country with which it comes into competition. And we see that this is the standard of perfection attained under nature. The endemic productions of New Zealand, for instance, are perfect one compared with another; but they are now rapidly yielding before the advancing legions of plants and animals introduced from Europe. Natural selection will not produce absolute

perfection, nor do we always meet, as far as we can judge, with this high standard under nature. The correction for the aberration of light is said by Müller not to be perfect even in that most perfect organ, the human eye. If our reason leads us to admire with enthusiasm a multitude of inimitable contrivances in nature, this same reason tells us, though we may easily err on both sides, that some other contrivances are less perfect. Can we consider the sting of the bee as perfect, which, when used against many kinds of enemies, cannot be withdrawn, owing to the backward serratures, and thus inevitably causes the death of the insect by tearing out its viscera?

If we look at the sting of the bee, as having existed in a remote progenitor, as a boring and serrated instrument, like that in so many members of the same great order, and that it has since been modified but not perfected for its present purpose, with the poison originally adapted for some other object, such as to produce galls, since intensified, we can perhaps understand how it is that the use of the sting should so often cause the insect's own death: for if on the whole the power of stinging be useful to the social community, it will fulfil all the requirements of natural selection, though it may cause the death of some few members. If we admire the several ingenious contrivances, by which orchids and many other plants are fertilised through insect agency, can we consider as equally perfect the elaboration of dense clouds of pollen by our fir trees, so that a few granules may be wafted by chance on to the ovules?

Summary: the Law of Unity of Type and of the Conditions of Existence embraced by the Theory of Natural Selection

We have seen in this chapter how cautious we should be in concluding that the most different habits of life could not graduate into each other; that a bat, for instance, could not have been formed by natural selection from an animal which at first only glided through the air.

We have seen that a species under new conditions of life may change its habits; or it may have diversified habits, with some very unlike those of its nearest congeners. Hence we can understand, bearing in mind that each organic being is trying to live wherever it can live, how it has arisen that there are upland geese with webbed feet, ground woodpeckers, diving thrushes, and petrels with the habits of auks.

Although the belief that an organ so perfect as the eye could have been formed by natural selection is enough to stagger anyone; yet in the case of any organ, if we know of a long series of gradations in complexity, each good for its possessor, then, under changing conditions of life, there is no logical impossibility in the acquirement of any conceivable degree of perfection through natural selection. In the cases in which we know of no intermediate or transitional states, we should be extremely cautious in concluding that none can have existed, for the metamorphoses of many organs show what wonderful changes in function are at least possible. For

instance, a swim bladder has apparently been converted into an air-breathing lung.

VII. MISCELLANEOUS OBJECTIONS TO THE THEORY OF NATURAL SELECTION

A DISTINGUISHED ZOOLOGIST, Mr. St. George Mivart, has recently collected all the objections which have ever been advanced by myself and others against the theory of natural selection, as propounded by Mr. Wallace and myself, and has illustrated them with admirable art and force. When thus marshalled, they make a formidable array; and as it forms no part of Mr. Mivart's plan to give the various facts and considerations opposed to his conclusions, no slight effort of reason and memory is left to the reader, who may wish to weigh the evidence on both sides.

All Mr. Mivart's objections will be, or have been, considered in the present volume. The one new point which appears to have struck many readers is, "that natural selection is incompetent to account for the incipient stages of useful structures." This subject is intimately connected with that of the gradation of characters, often accompanied by a change of function—for instance, the conversion of a swim bladder into lungs—points which were discussed in the last chapter. Nevertheless, I will here consider in some detail several of the cases advanced by Mr. Mivart, selecting those which are the most illustrative, as want of space prevents me from considering all.

The giraffe, by its lofty stature, much elongated neck, forelegs, head and tongue, has its whole frame beautifully adapted for browsing on the higher branches of trees. It can thus obtain food beyond the reach of the other Ungulata or hoofed animals inhabiting the same country; and this must be a great advantage to it during dearths. Before coming to Mr. Mivart's objections, it may be well to explain once again how natural selection will act in all ordinary cases. Man has modified some of his animals, without necessarily having attended to special points of structure, by simply preserving and breeding from the fleetest individuals, as with the race horse and greyhound, or as with the gamecock, by breeding from the victorious birds. So under nature with the nascent giraffe the individuals which were the highest browsers, and were able during dearths to reach even an inch or two above the others, will often have been preserved; for they will have roamed over the whole country in search of food. That the individuals of the same species often differ slightly in the relative lengths of all their parts may be seen in many works of natural history, in which careful measurements are given. These slight proportional differences, due to the laws of growth and variation, are not of the slightest use or importance to most species. But it will have been otherwise with the nascent giraffe, considering its probable habits of life; for those individuals which had some one part or several parts of their bodies rather more elongated than usual would generally have survived. These

will have intercrossed and left offspring, either inheriting the same bodily peculiarities or with a tendency to vary again in the same manner; whilst the individuals, less favoured in the same respects, will have been the most liable to perish.

We here see that there is no need to separate single pairs, as man does, when he methodically improves a breed: natural selection will preserve and thus separate all the superior individuals, allowing them freely to intercross, and will destroy all the inferior individuals. By this process long-continued, which exactly corresponds with what I have called unconscious selection by man, combined no doubt in a most important manner with the inherited effects of the increased use of parts, it seems to me almost certain that an ordinary hoofed quadruped might be converted into a giraffe.

To this conclusion Mr. Mivart brings forward two objections. One is that the increased size of the body would obviously require an increased supply of food, and he considers it as "very problematical whether the disadvantages thence arising would not, in times of scarcity, more than counterbalance the advantages." But as the giraffe does actually exist in large numbers in South Africa, and as some of the largest antelopes in the world, taller than an ox, abound there, why should we doubt that, as far as size is concerned, intermediate gradations could formerly have existed there, subjected as now to severe dearths? Assuredly the being able to reach, at each stage of increased size, to a supply of food, left untouched by the other hoofed quadrupeds of the country, would have been of some advantage to the nascent giraffe. Nor must we overlook the fact that increased bulk would act as a protection against almost all beasts of prey excepting the lion; and against this animal, its tall neck—and the taller the better—would, as Mr. Chauncey Wright has remarked, serve as a watchtower. It is from this cause, as Sir Samuel Baker remarks, that no animal is more difficult to stalk than the giraffe. This animal also uses its long neck as a means of offence or defence, by violently swinging his head armed with stump-like horns. The preservation of each species can rarely be determined by any one advantage, but by the union of all, great and small.

Mr. Mivart then asks (and this is his second objection), if natural selection be so potent, and if high browsing be so great an advantage, why has not any other hoofed quadruped acquired a long neck and lofty stature, besides the giraffe, and, in a lesser degree, the camel, guanaco, and macrauchenia? Or, again, why has not any member of the group acquired a long proboscis? With respect to South Africa, which was formerly inhabited by numerous herds of the giraffe, the answer is not difficult, and can best be given by an illustration. In every meadow in England in which trees grow, we see the lower branches trimmed or planed to an exact level by the browsing of the horses or cattle; and what advantage would it be, for instance, to sheep, if kept there, to acquire slightly longer necks? In every district some one kind of animal will almost certainly be able to browse higher than the others; and it is almost equally certain that this

one kind alone could have its neck elongated for this purpose, through natural selection and the effects of increased use. In South Africa the competition for browsing on the higher branches of the acacias and other trees must be between giraffe and giraffe, and not with the other ungulate animals.

Why, in other quarters of the world, various animals belonging to this same order have not acquired either an elongated neck or a proboscis cannot be distinctly answered; but it is as unreasonable to expect a distinct answer to such a question as why some event in the history of mankind did not occur in one country whilst it did in another. We are ignorant with respect to the conditions which determine the numbers and range of each species; and we cannot even conjecture what changes of structure would be favourable to its increase in some new country. We can, however, see in a general manner that various causes might have interfered with the development of a long neck or proboscis. To reach the foliage at a considerable height (without climbing, for which hoofed animals are singularly ill-constructed) implies greatly increased bulk of body; and we know that some areas support singularly few large quadrupeds, for instance South America, though it is so luxuriant; whilst South Africa abounds with them to an unparalleled degree. Why this should be so, we do not know; nor why the later tertiary periods should have been so much more favourable for their existence than the present time. Whatever the causes may have been, we can see that certain districts and times would have been much more favourable than others for the development of so large a quadruped as the giraffe.

The mammary glands are common to the whole class of mammals, and are indispensable for their existence; they must, therefore, have been developed at an extremely remote period, and we can know nothing positively about their manner of development. Mr. Mivart asks: "Is it conceivable that the young of any animal was ever saved from destruction by accidentally sucking a drop of scarcely nutritious fluid from an accidentally hypertrophied cutaneous gland of its mother? And even if one was so, what chance was there of the perpetuation of such a variation?" But the case is not here put fairly. It is admitted by most evolutionists that mammals are descended from a marsupial form; and if so, the mammary glands will have been at first developed within the marsupial sack. In the case of the fish (Hippocampus) the eggs are hatched, and the young are reared for a time, within a sack of this nature; and an American naturalist, Mr. Lockwood, believes from what he has seen of the development of the young, that they are nourished by a secretion from the cutaneous glands of the sack. Now with the early progenitors of mammals, almost before they deserved to be thus designated, is it not at least possible that the young might have been similarly nourished? And in this case, the individuals which secreted a fluid, in some degree or manner the most nutritious, so as to partake of the nature of milk, would in the long run have reared a larger number of well-nourished offspring, than would the individuals which secreted a poorer fluid; and thus the cutaneous

glands, which are the homologues of the mammary glands, would have been improved or rendered more effective. It accords with the widely extended principle of specialisation that the glands over a certain space of the sack should have become more highly developed than the remainder; and they would then have formed a breast, but at first without a nipple, as we see in the Ornithorhyncus, at the base of the mammalian series. Through what agency the glands over a certain space became more highly specialised than the others, I will not pretend to decide, whether in part through compensation of growth, the effects of use, or of natural selection.

The development of the mammary glands would have been of no service, and could not have been effected through natural selection, unless the young at the same time were able to partake of the secretion. There is no greater difficulty in understanding how young mammals have instinctively learnt to suck the breast than in understanding how unhatched chickens have learnt to break the eggshell by tapping against it with their specially adapted beaks; or how a few hours after leaving the shell they have learnt to pick up grains of food. In such cases the most probable solution seems to be that the habit was at first acquired by practice at a more advanced age, and afterwards transmitted to the offspring at an earlier age.

In the vegetable kingdom Mr. Mivart only alludes to two cases, namely the structure of the flowers of orchids, and the movements of climbing plants. With respect to the former, he says, "the explanation of their *origin* is deemed thoroughly unsatisfactory—utterly insufficient to explain the incipient, infinitesimal beginnings of structures which are of utility only when they are considerably developed." I have fully treated this subject in another work.

We will now turn to climbing plants. These can be arranged in a long series, from those which simply twine round a support, to those which I have called leaf climbers, and to those provided with tendrils. In these two latter classes the stems have generally, but not always, lost the power of twining, though they retain the power of revolving, which the tendrils likewise possess. The gradations from leaf climbers to tendril bearers are wonderfully close, and certain plants may be indifferently placed in either class. But in ascending the series from simple twiners to leaf climbers, an important quality is added, namely sensitiveness to a touch, by which means the footstalks of the leaves or flowers, or these modified and converted into tendrils, are excited to bend round and clasp the touching object. He who will read my memoir on these plants will, I think, admit that all the many gradations in function and structure between simple twiners and tendril bearers are in each case beneficial in a high degree to the species. For instance, it is clearly a great advantage to a twining plant to become a leaf climber; and it is probable that every twiner which possessed leaves with long footstalks would have been developed into a leaf climber if the footstalks had possessed in any slight degree the requisite sensitiveness to a touch.

As twining is the simplest means of ascending a support, and forms

the basis of our series, it may naturally be asked how did plants acquire this power in an incipient degree, afterwards to be improved and increased through natural selection? The power of twining depends, firstly, on the stems whilst young being extremely flexible (but this is a character common to many plants which are not climbers); and, secondly, on their continually bending to all points of the compass, one after the other in succession, in the same order. By this movement the stems are inclined to all sides, and are made to move round and round. As soon as the lower part of a stem strikes against any object and is stopped, the upper part still goes on bending and revolving, and thus necessarily twines round and up the support. The revolving movement ceases after the early growth of each shoot. As in many widely separated families of plants, single species and single genera possess the power of revolving, and have thus become twiners, they must have independently acquired it, and cannot have inherited it from a common progenitor. Hence I was led to predict that some slight tendency to a movement of this kind would be found to be far from uncommon with plants which did not climb; and that this had afforded the basis for natural selection to work on and improve. When I made this prediction, I knew of only one imperfect case, namely, of the young flower peduncles of a Maurandia which revolved slightly and irregularly, like the stems of twining plants, but without making any use of this habit. Soon afterwards Fritz Müller discovered that the young stems of an Alisma and of a Linum—plants which do not climb and are widely separated in the natural system—revolved plainly, though irregularly; and he states that he has reason to suspect that this occurs with some other plants. These slight movements appear to be of no service to the plants in question; anyhow, they are not of the least use in the way of climbing, which is the point that concerns us. Nevertheless we can see that if the stems of these plants had been flexible, and if under the conditions to which they are exposed it had profited them to ascend to a height, then the habit of slightly and irregularly revolving might have been increased and utilised through natural selection, until they had become converted into well-developed twining species.

Mr. Mivart is further inclined to believe, and some naturalists agree with him, that new species manifest themselves "with suddenness and by modifications appearing at once." For instance, he supposes that the differences between the extinct three-toed Hipparion and the horse arose suddenly. He thinks it difficult to believe that the wing of a bird "was developed in any other way than by a comparatively sudden modification of a marked and important kind"; and apparently he would extend the same view to the wings of bats and pterodactyles. This conclusion, which implies great breaks or discontinuity in the series, appears to me improbable in the highest degree.

My reasons for doubting whether natural species have changed as abruptly as have occasionally domestic races, and for entirely disbelieving that they have changed in the wonderful manner indicated by Mr. Mivart, are as follows. According to our experience, abrupt and strongly

marked variations occur in our domesticated productions, singly and at rather long intervals of time. If such occurred under nature, they would be liable, as formerly explained, to be lost by accidental causes of destruction and by subsequent intercrossing; and so it is known to be under domestication, unless abrupt variations of this kind are specially preserved and separated by the care of man. Hence in order that a new species should suddenly appear in the manner supposed by Mr. Mivart, it is almost necessary to believe, in opposition to all analogy, that several wonderfully changed individuals appeared simultaneously within the same district. This difficulty, as in the case of unconscious selection by man, is avoided on the theory of gradual evolution, through the preservation of a large number of individuals, which varied more or less in any favourable direction, and of the destruction of a large number which varied in an opposite manner.

VIII. INSTINCT

MANY INSTINCTS are so wonderful that their development will probably appear to the reader a difficulty sufficient to overthrow my whole theory. I may here premise that I have nothing to do with the origin of the mental powers, any more than I have with that of life itself. We are concerned only with the diversities of instinct and of the other mental faculties in animals of the same class.

I will not attempt any definition of instinct. It would be easy to show that several distinct mental actions are commonly embraced by this term; but everyone understands what is meant when it is said that instinct impels the cuckoo to migrate and to lay her eggs in other birds' nests. An action which we ourselves require experience to enable us to perform, when performed by an animal, more especially by a very young one, without experience, and when performed by many individuals in the same way, without their knowing for what purpose it is performed, is usually said to be instinctive.

It will be universally admitted that instincts are as important as corporeal structures for the welfare of each species, under its present conditions of life. Under changed conditions of life, it is at least possible that slight modifications of instinct might be profitable to a species; and if it can be shown that instincts do vary ever so little, then I can see no difficulty in natural selection preserving and continually accumulating variations of instinct to any extent that was profitable. It is thus, as I believe, that all the most complex and wonderful instincts have originated. As modifications of corporeal structure arise from, and are increased by, use or habit, and are diminished or lost by disuse, so I do not doubt it has been with instincts. But I believe that the effects of habit are in many cases of subordinate importance to the effects of the natural selection of what may be called spontaneous variations of instincts;—that is of variations produced by the same unknown causes which produce slight deviations of bodily structure.

No complex instinct can possibly be produced through natural selection, except by the slow and gradual accumulation of numerous slight, yet profitable, variations. Hence, as in the case of corporeal structures, we ought to find in nature, not the actual transitional gradations by which each complex instinct has been acquired—for these could be found only in the lineal ancestors of each species—but we ought to find in the collateral lines of descent some evidence of such gradations; or we ought at least to be able to show that gradations of some kind are possible; and this we certainly can do. I have been surprised to find, making allowance for the instincts of animals having been but little observed except in Europe and North America, and for no instinct being known amongst extinct species, how very generally gradations, leading to the most complex instincts, can be discovered.

Again, as in the case of corporeal structure, and conformably to my theory, the instinct of each species is good for itself, but has never, as far as we can judge, been produced for the exclusive good of others. One of the strongest instances of an animal apparently performing an action for the sole good of another, with which I am acquainted, is that of aphides voluntarily yielding, as was first observed by Huber, their sweet excretion to ants: that they do so voluntarily, the following facts show. I removed all the ants from a group of about a dozen aphides on a dock plant, and prevented their attendance during several hours. After this interval, I felt sure that the aphides would want to excrete. I watched them for some time through a lens, but not one excreted; I then tickled and stroked them with a hair in the same manner, as well as I could, as the ants do with their antennæ; but not one excreted. Afterwards I allowed an ant to visit them, and it immediately seemed, by its eager way of running about, to be well aware what a rich flock it had discovered; it then began to play with its antennæ on the abdomen first of one aphis and then of another; and each, as soon as it felt the antennæ, immediately lifted up its abdomen and excreted a limpid drop of sweet juice, which was eagerly devoured by the ant. Even the quite young aphides behaved in this manner, showing that the action was instinctive, and not the result of experience. It is certain, from the observations of Huber, that the aphides show no dislike to the ants: if the latter be not present they are at last compelled to eject their excretion. But as the excretion is extremely viscid, it is no doubt a convenience to the aphides to have it removed; therefore probably they do not excrete solely for the good of the ants.

As some degree of variation in instincts under a state of nature, and the inheritance of such variations, is indispensable for the action of natural selection, as many instances as possible ought to be given; but want of space prevents me. I can only assert that instincts certainly do vary—for instance, the migratory instinct, both in extent and direction, and in its total loss. So it is with the nests of birds, which vary partly in dependence on the situations chosen, and on the nature and temperature of the country inhabited, but often from causes wholly unknown to us: Audubon has

given several remarkable cases of differences in the nests of the same species in the northern and southern United States.

Inherited Changes of Habit or Instinct in Domesticated Animals

The possibility, or even probability, of inherited variations of instinct in a state of nature will be strengthened by briefly considering a few cases under domestication. We shall thus be enabled to see the part which habit and the selection of so-called spontaneous variations have played in modifying the mental qualities of our domestic animals. It is notorious how much domestic animals vary in their mental qualities. With cats, for instance, one naturally takes to catching rats, and another mice, and these tendencies are known to be inherited. But let us look to the familiar case of the breeds of the dogs: it cannot be doubted that young pointers (I have myself seen a striking instance) will sometimes point and even back other dogs the very first time that they are taken out; retrieving is certainly in some degree inherited by retrievers; and a tendency to run round, instead of at, a flock of sheep, by shepherd dogs. I cannot see that these actions, performed without experience by the young, and in nearly the same manner by each individual, performed with eager delight by each breed, and without the end being known—for the young pointer can no more know that he points to aid his master than the white butterfly knows why she lays her eggs on the leaf of the cabbage—I cannot see that these actions differ essentially from true instincts. If we were to behold one kind of wolf, when young and without any training, as soon as it scented its prey, stand motionless like a statue, and then slowly crawl forward with a peculiar gait; and another kind of wolf rushing round, instead of at, a herd of deer, and driving them to a distant point, we should assuredly call these actions instinctive. Domestic instincts, as they may be called, are certainly far less fixed than natural instincts; but they have been acted on by far less rigorous selection, and have been transmitted for an incomparably shorter period, under less fixed conditions of life.

Natural instincts are lost under domestication: a remarkable instance of this is seen in those breeds of fowls which very rarely or never become "broody," that is, never wish to sit on their eggs. Familiarity alone prevents our seeing how largely and how permanently the minds of our domestic animals have been modified. It is scarcely possible to doubt that the love of man has become instinctive in the dog. All wolves, foxes, jackals, and species of the cat genus, when kept tame, are most eager to attack poultry, sheep, and pigs; and this tendency has been found incurable in dogs which have been brought home as puppies from countries such as Tierra del Fuego and Australia, where the savages do not keep these domestic animals. How rarely, on the other hand, do our civilised dogs, even when quite young, require to be taught not to attack poultry, sheep, and pigs! No doubt they occasionally do make an attack, and are then beaten;

and if not cured, they are destroyed; so that habit and some degree of selection have probably concurred in civilising by inheritance our dogs.

Hence we may conclude that under domestication instincts have been acquired, and natural instincts have been lost, partly by habit, and partly by man selecting and accumulating, during successive generations, peculiar mental habits and actions, which at first appeared from what we must in our ignorance call an accident. In some cases compulsory habit alone has sufficed to produce inherited mental changes; in other cases, compulsory habit has done nothing, and all has been the result of selection, pursued both methodically and unconsciously: but in most cases habit and selection have probably concurred.

Special Instincts

We shall, perhaps, best understand how instincts in a state of nature have become modified by considering the slave-making instinct of certain ants. This remarkable instinct was first discovered in the Formica (Polyerges) rufescens by Pierre Huber, a better observer even than his celebrated father. This ant is absolutely dependent on its slaves; without their aid, the species would certainly become extinct in a single year. The males and fertile females do no work of any kind, and the workers or sterile females, though most energetic and courageous in capturing slaves, do no other work. They are incapable of making their own nests, or of feeding their own larvæ. When the old nest is found inconvenient, and they have to migrate, it is the slaves which determine the migration, and actually carry their masters in their jaws. So utterly helpless are the masters that when Huber shut up thirty of them without a slave, but with plenty of the food which they like best, and with their own larvæ and pupæ to stimulate them to work, they did nothing; they could not even feed themselves, and many perished of hunger. Huber then introduced a single slave (F. fusca), and she instantly set to work, fed and saved the survivors; made some cells and tended the larvæ, and put all to rights. What can be more extraordinary than these well-ascertained facts? If we had not known of any other slave-making ant, it would have been hopeless to speculate how so wonderful an instinct could have been perfected.

Another species, Formica sanguinea, was likewise first discovered by P. Huber to be a slave-making ant. This species is found in the southern parts of England, and its habits have been attended to by Mr. F. Smith, of the British Museum, to whom I am much indebted for information on this and other subjects. Although fully trusting to the statements of Huber and Mr. Smith, I tried to approach the subject in a sceptical frame of mind, as anyone may well be excused for doubting the existence of so extraordinary an instinct as that of making slaves. Hence, I will give the observations which I made in some little detail. I opened fourteen nests of F. sanguinea, and found a few slaves in all. Males and fertile females of the slave species (F. fusca) are found only in their own proper com-

munities, and have never been observed in the nests of F. sanguinea. The slaves are black and not above half the size of their red masters, so that the contrast in their appearance is great. When the nest is slightly disturbed, the slaves occasionally come out, and like their masters are much agitated and defend the nest: when the nest is much disturbed, and the larvæ and pupæ are exposed, the slaves work energetically together with their masters in carrying them away to a place of safety. Hence it is clear that the slaves feel quite at home.

One day I fortunately witnessed a migration of F. sanguinea from one nest to another, and it was a most interesting spectacle to behold the masters carefully carrying their slaves in their jaws instead of being carried by them, as in the case of F. rufescens. Another day my attention was struck by about a score of the slave makers haunting the same spot, and evidently not in search of food; they approached and were vigorously repulsed by an independent community of the slave species (F. fusca); sometimes as many as three of these ants clinging to the legs of the slave-making F. sanguinea. The latter ruthlessly killed their small opponents, and carried their dead bodies as food to their nest, twenty-nine yards distant; but they were prevented from getting any pupæ to rear as slaves. I then dug up a small parcel of the pupæ of F. fusca from another nest, and put them down on a bare spot near the place of combat; they were eagerly seized and carried off by the tyrants, who perhaps fancied that, after all, they had been victorious in their late combat.

One evening I visited another community of F. sanguinea, and found a number of these ants returning home and entering their nests, carrying the dead bodies of F. fusca (showing that it was not a migration) and numerous pupæ. I traced a long file of ants burthened with booty, for about forty yards back, to a very thick clump of heath, whence I saw the last individual of F. sanguinea emerge, carrying a pupa; but I was not able to find the desolated nest in the thick heath. The nest, however, must have been close at hand, for two or three individuals of F. fusca were rushing about in the greatest agitation, and one was perched motionless with its own pupa in its mouth on the top of a spray of heath, an image of despair over its ravaged home.

Such are the facts, though they did not need confirmation by me, in regard to the wonderful instinct of making slaves. Let it be observed what a contrast the instinctive habits of F. sanguinea present with those of the continental F. rufescens. The latter does not build its own nest, does not determine its own migrations, does not collect food for itself or its young, and cannot even feed itself: it is absolutely dependent on its numerous slaves. Formica sanguinea, on the other hand, possesses much fewer slaves, and in the early part of the summer extremely few: the masters determine when and where a new nest shall be formed, and when they migrate, the masters carry the slaves. Both in Switzerland and England the slaves seem to have the exclusive care of the larvæ, and the masters alone go on slave-making expeditions. In Switzerland the slaves and masters work together, making and bringing materials for the nest; both, but

chiefly the slaves, tend, and milk, as it may be called, their aphides; and thus both collect food for the community. In England the masters alone usually leave the nest to collect building materials and food for themselves, their slaves and larvæ. So that the masters in this country receive much less service from their slaves than they do in Switzerland.

By what steps the instinct of F. sanguinea originated I will not pretend to conjecture. But as ants which are not slave makers will, as I have seen, carry off the pupæ of other species, if scattered near their nests, it is possible that such pupæ originally stored as food might become developed; and the foreign ants thus unintentionally reared would then follow their proper instincts, and do what work they could. If their presence proved useful to the species which had seized them—if it were more advantageous to this species to capture workers than to procreate them—the habit of collecting pupæ, originally for food, might by natural selection be strengthened and rendered permanent for the very different purpose of raising slaves. When the instinct was once acquired, if carried out to a much less extent even than in our British F. sanguinea, which, as we have seen, is less aided by its slaves than the same species in Switzerland, natural selection might increase and modify the instinct—always supposing each modification to be of use to the species—until an ant was formed as abjectly dependent on its slaves as is the Formica rufescens.

No doubt many instincts of very difficult explanation could be opposed to the theory of natural selection—cases in which we cannot see how an instinct could have originated; cases in which no intermediate gradations are known to exist; cases of instincts of such trifling importance that they could hardly have been acted on by natural selection; cases of instincts almost identically the same in animals so remote in the scale of nature that we cannot account for their similarity by inheritance from a common progenitor, and consequently must believe that they were independently acquired through natural selection. I will not here enter on these several cases, but will confine myself to one special difficulty, which at first appeared to me insuperable, and actually fatal to the whole theory. I allude to the neuters or sterile females in insect communities; for these neuters often differ widely in instinct and in structure from both the males and fertile females, and yet, from being sterile, they cannot propagate their kind.

The subject well deserves to be discussed at great length, but I will here take only a single case, that of working or sterile ants. How the workers have been rendered sterile is a difficulty; but not much greater than that of any other striking modification of structure; for it can be shown that some insects and other articulate animals in a state of nature occasionally become sterile; and if such insects had been social, and it had been profitable to the community that a number should have been annually born capable of work, but incapable of procreation, I can see no especial difficulty in this having been effected through natural selection. But I must pass over this preliminary difficulty. The great difficulty lies in the working ants differing widely from both the males and the fertile

females in structure, as in the shape of the thorax, and in being destitute of wings and sometimes of eyes, and in instinct. As far as instinct alone is concerned, the wonderful difference in this respect between the workers and the perfect females would have been better exemplified by the hive bee. If a working ant or other neuter insect had been an ordinary animal, I should have unhesitatingly assumed that all its characters had been slowly acquired through natural selection; namely, by individuals having been born with slight profitable modifications, which were inherited by the offspring; and that these again varied and again were selected, and so onwards. But with the working ant we have an insect differing greatly from its parents, yet absolutely sterile; so that it could never have transmitted successively acquired modifications of structure or instinct to its progeny. It may well be asked how is it possible to reconcile this case with the theory of natural selection?

First, let it be remembered that we have innumerable instances, both in our domestic productions and in those in a state of nature, of all sorts of differences of inherited structure which are correlated with certain ages, and with either sex. We have differences correlated not only with one sex, but with that short period when the reproductive system is active, as in the nuptial plumage of many birds, and in the hooked jaws of the male salmon. We have even slight differences in the horns of different breeds of cattle in relation to an artificially imperfect state of the male sex; for oxen of certain breeds have longer horns than the oxen of other breeds, relatively to the length of the horns in both the bulls and cows of these same breeds. Hence I can see no great difficulty in any character becoming correlated with the sterile condition of certain members of insect communities: the difficulty lies in understanding how such correlated modifications of structure could have been slowly accumulated by natural selection.

This difficulty, though appearing insuperable, is lessened, or, as I believe, disappears, when it is remembered that selection may be applied to the family, as well as to the individual, and may thus gain the desired end. Breeders of cattle wish the flesh and fat to be well marbled together: an animal thus characterised has been slaughtered, but the breeder has gone with confidence to the same stock and has succeeded. Such faith may be placed in the power of selection that a breed of cattle, always yielding oxen with extraordinarily long horns, could, it is probable, be formed by carefully watching which individual bulls and cows, when matched, produced oxen with the longest horns; and yet no ox would ever have propagated its kind. Here is a better and real illustration: according to M. Verlot, some varieties of the double annual stock, from having been long and carefully selected to the right degree, always produce a large proportion of seedlings bearing double and quite sterile flowers; but they likewise yield some single and fertile plants. These latter, by which alone the variety can be propagated, may be compared with the fertile male and female ants, and the double sterile plants with the neuters of the same community. As with the varieties of the stock, so with social insects, selection has

been applied to the family, and not to the individual, for the sake of gaining a serviceable end. Hence we may conclude that slight modifications of structure or of instinct, correlated with the sterile condition of certain members of the community, have proved advantageous: consequently the fertile males and females have flourished, and transmitted to their fertile offspring a tendency to produce sterile members with the same modifications. This process must have been repeated many times, until that prodigious amount of difference between the fertile and sterile females of the same species has been produced which we see in many social insects.

But we have not as yet touched on the acme of the difficulty; namely, the fact that the neuters of several ants differ, not only from the fertile females and males, but from each other, sometimes to an almost incredible degree, and are thus divided into two or even three castes. The castes, moreover, do not commonly graduate into each other, but are perfectly well defined; being as distinct from each other as are any two species of the same genus, or rather as any two genera of the same family. Thus in Eciton, there are working and soldier neuters, with jaws and instincts extraordinarily different: in Cryptocerus, the workers of one caste alone carry a wonderful sort of shield on their heads, the use of which is quite unknown: in the Mexican Myrmecocystus, the workers of one caste never leave the nest; they are fed by the workers of another caste, and they have an enormously developed abdomen which secretes a sort of honey, supplying the place of that excreted by the aphides, or the domestic cattle as they may be called, which our European ants guard and imprison.

It will indeed be thought that I have an overweening confidence in the principle of natural selection, when I do not admit that such wonderful and well-established facts at once annihilate the theory. In the simpler case of neuter insects all of one caste, which, as I believe, have been rendered different from the fertile males and females through natural selection, we may conclude from the analogy of ordinary variations that the successive, slight, profitable modifications did not first arise in all the neuters in the same nest, but in some few alone; and that by the survival of the communities with females which produced most neuters having the advantageous modifications, all the neuters ultimately came to be thus characterised. According to this view we ought occasionally to find in the same nest neuter insects, presenting gradations of structure; and this we do find, even not rarely, considering how few neuter insects out of Europe have been carefully examined. Mr. F. Smith has shown that the neuters of several British ants differ surprisingly from each other in size and sometimes in colour; and that the extreme forms can be linked together by individuals taken out of the same nest: I have myself compared perfect gradations of this kind. It sometimes happens that the larger or the smaller sized workers are the most numerous; or that both large and small are numerous, whilst those of an intermediate size are scanty in numbers. Formica flava has larger and smaller workers, with some few of intermediate size; and, in this species, as Mr. F. Smith has observed, the larger workers have simple eyes (ocelli), which though small can be plainly dis-

tinguished, whereas the smaller workers have their ocelli rudimentary. Having carefully dissected several specimens of these workers, I can affirm that the eyes are far more rudimentary in the smaller workers than can be accounted for merely by their proportionately lesser size; and I fully believe, though I dare not assert so positively, that the workers of intermediate size have their ocelli in an exactly intermediate condition. So that here we have two bodies of sterile workers in the same nest, differing not only in size, but in their organs of vision, yet connected by some few members in an intermediate condition.

I may give one other case: so confidently did I expect occasionally to find gradations of important structures between the different castes of neuters in the same species that I gladly availed myself of Mr. F. Smith's offer of numerous specimens from the same nest of the driver ant (Anomma) of West Africa. The reader will perhaps best appreciate the amount of difference in these workers by my giving not the actual measurements, but a strictly accurate illustration: the difference was the same as if we were to see a set of workmen building a house, of whom many were five feet four inches high and many sixteen feet high; but we must in addition suppose that the larger workmen had heads four instead of three times as big as those of the smaller men, and jaws nearly five times as big. The jaws, moreover, of the working ants of the several sizes differed wonderfully in shape and in the form and number of the teeth. But the important fact for us is that, though the workers can be grouped into castes of different size, yet they graduate insensibly into each other, as does the widely different structure of their jaws. I speak confidently on this latter point, as Sir J. Lubbock made drawings for me, with the camera lucida, of the jaws which I dissected from the workers of the several sizes. Mr. Bates, in his interesting *Naturalist on the Amazons,* has described analogous cases.

With these facts before me, I believe that natural selection, by acting on the fertile ants or parents, could form a species which should regularly produce neuters, all of large size with one form of jaw, or all of small size with widely different jaws; or lastly, and this is the greatest difficulty, one set of workers of one size and structure, and simultaneously another set of workers of a different size and structure;—a graduated series having first been formed, as in the case of the driver ant, and then the extreme forms having been produced in greater and greater numbers, through the survival of the parents which generated them, until none with an intermediate structure were produced.

I do not pretend that the facts given in this chapter strengthen in any great degree my theory; but none of the cases of difficulty, to the best of my judgment, annihilate it. On the other hand, the fact that instincts are not always absolutely perfect and are liable to mistakes:—that no instinct can be shown to have been produced for the good of other animals, though animals take advantage of the instincts of others;—that the canon in natural history, of "Natura non facit saltum," is applicable to instincts as well as to corporeal structure, and is plainly explicable on the foregoing

views, but is otherwise inexplicable—all tend to corroborate the theory of natural selection.

This theory is also strengthened by some few other facts in regard to instincts; as by that common case of closely allied but distinct species, when inhabiting distant parts of the world and living under considerably different conditions of life, yet often retaining nearly the same instincts. For instance, we can understand, on the principle of inheritance, how it is that the thrush of tropical South America lines its nest with mud, in the same peculiar manner as does our British thrush; how it is that the hornbills of Africa and India have the same extraordinary instinct of plastering up and imprisoning the females in a hole in a tree, with only a small hole left in the plaster through which the males feed them and their young when hatched; how it is that the male wrens (Troglodytes) of North America build "cock nests," to roost in, like the males of our kitty-wrens—a habit wholly unlike that of any other known bird. Finally, it may not be a logical deduction, but to my imagination it is far more satisfactory to look at such instincts as the young cuckoo ejecting its foster brothers—ants making slaves—the larvæ of ichneumonidæ feeding within the live bodies of caterpillars—not as specially endowed or created instincts, but as small consequences of one general law leading to the advancement of all organic beings—namely, multiply, vary, let the strongest live and the weakest die.

IX. ON THE IMPERFECTION OF THE GEOLOGICAL RECORD

THE MAIN CAUSE of innumerable intermediate links not now occurring everywhere throughout nature depends on the very process of natural selection, through which new varieties continually take the places of and supplant their parent forms. But just in proportion as this process of extermination has acted on an enormous scale, so must the number of intermediate varieties, which have formerly existed, be truly enormous. Why then is not every geological formation and every stratum full of such intermediate links? Geology assuredly does not reveal any such finely graduated organic chain; and this, perhaps, is the most obvious and serious objection which can be urged against the theory. The explanation lies, as I believe, in the extreme imperfection of the geological record.

In the first place, it should always be borne in mind what sort of intermediate forms must, on the theory, have formerly existed. I have found it difficult, when looking at any two species, to avoid picturing to myself forms *directly* intermediate between them. But this is a wholly false view; we should always look for forms intermediate between each species and a common but unknown progenitor; and the progenitor will generally have differed in some respects from all its modified descendants. To give a simple illustration: If we look to forms very distinct, for instance, to the horse and tapir, we have no reason to suppose that links directly intermediate between them ever existed, but between each and an un-

known common parent. The common parent will have had in its whole organisation much general resemblance to the tapir and to the horse; but in some points of structure may have differed considerably from both, even perhaps more than they differ from each other. Hence, in all such cases, we should be unable to recognise the parent form of any two or more species, even if we closely compared the structure of the parent with that of its modified descendants, unless at the same time we had a nearly perfect chain of the intermediate links.

On the Poorness of Palæontological Collections

Now let us turn to our richest geological museums, and what a paltry display we behold! That our collections are imperfect is admitted by everyone. The remark of that admirable palæontologist, Edward Forbes, should never be forgotten, namely, that very many fossil species are known and named from single and often broken specimens, or from a few specimens collected on some one spot. Only a small portion of the surface of the earth has been geologically explored, and no part with sufficient care, as the important discoveries made every year in Europe prove. No organism wholly soft can be preserved. Shells and bones decay and disappear when left on the bottom of the sea, where sediment is not accumulating. In regard to mammiferous remains, a glance at the historical table published in Lyell's *Manual* will bring home the truth, how accidental and rare is their preservation, far better than pages of detail. Nor is their rarity surprising, when we remember how large a proportion of the bones of tertiary mammals have been discovered either in caves or in lacustrine deposits; and that not a cave or true lacustrine bed is known belonging to the age of our secondary or palæozoic formations.

But the imperfection in the geological record largely results from another and more important cause than any of the foregoing; namely, from the several formations being separated from each other by wide intervals of time. This doctrine has been emphatically admitted by many geologists and palæontologists, who, like E. Forbes, entirely disbelieve in the change of species. When we see the formations tabulated in written works, or when we follow them in nature, it is difficult to avoid believing that they are closely consecutive. But we know, for instance, from Sir R. Murchison's great work on Russia, what wide gaps there are in that country between the superimposed formations; so it is in North America, and in many other parts of the world. The most skilful geologist, if his attention had been confined exclusively to these large territories, would never have suspected that, during the periods which were blank and barren in his own country, great piles of sediment, charged with new and peculiar forms of life, had elsewhere been accumulated. And if, in each separate territory, hardly any idea can be formed of the length of time which has elapsed between the consecutive formations, we may infer that this could nowhere be ascertained. The frequent and great changes in the mineral-

ogical composition of consecutive formations, generally implying great changes in the geography of the surrounding lands, whence the sediment was derived, accord with the belief of vast intervals of time having elapsed between each formation.

We may, I think, conclude that sediment must be accumulated in extremely thick, solid, or extensive masses, in order to withstand the incessant action of the waves, when first upraised and during successive oscillations of level as well as the subsequent subaerial degradation. Such thick and extensive accumulations of sediment may be formed in two ways; either in profound depths of the sea, in which case the bottom will not be inhabited by so many and such varied forms of life, as the more shallow seas; and the mass when upraised will give an imperfect record of the organisms which existed in the neighbourhood during the period of its accumulation. Or sediment may be deposited to any thickness and extent over a shallow bottom, if it continue slowly to subside. In this latter case, as long as the rate of subsidence and the supply of sediment nearly balance each other, the sea will remain shallow and favourable for many and varied forms, and thus a rich fossiliferous formation, thick enough, when upraised, to resist a large amount of denudation, may be formed.

I am convinced that nearly all our ancient formations, which are throughout the greater part of their thickness *rich in fossils,* have thus been formed during subsidence.

All geological facts tell us plainly that each area has undergone slow oscillations of level, and apparently these oscillations have affected wide spaces. Consequently, formations rich in fossils, and sufficiently thick and extensive to resist subsequent degradation, will have been formed over wide spaces during periods of subsidence, but only where the supply of sediment was sufficient to keep the sea shallow and to embed and preserve the remains before they had time to decay. On the other hand, as long as the bed of the sea remains stationary, *thick* deposits cannot have been accumulated in the shallow parts, which are the most favourable to life. Still less can this have happened during the alternate periods of elevation; or, to speak more accurately, the beds which were then accumulated will generally have been destroyed by being upraised and brought within the limits of the coast action.

One remark is here worth a passing notice. During periods of elevation the area of the land and of the adjoining shoal parts of the sea will be increased, and new stations will often be formed:—all circumstances favourable for the formation of new varieties and species; but during such periods there will generally be a blank in the geological record. On the other hand, during subsidence, the inhabited area and number of inhabitants will decrease (excepting on the shores of a continent when first broken up into an archipelago), and consequently during subsidence, though there will be much extinction, few new varieties or species will be formed; and it is during these very periods of subsidence that the deposits which are richest in fossils have been accumulated.

On the Absence of Numerous Intermediate Varieties in any Single Formation

From these several considerations, it cannot be doubted that the geological record, viewed as a whole, is extremely imperfect; but if we confine our attention to any one formation, it becomes much more difficult to understand why we do not therein find closely graduated varieties between the allied species which lived at its commencement and at its close. Several cases are on record of the same species presenting varieties in the upper and lower parts of the same formation; thus, Trautschold gives a number of instances with Ammonites; and Hilgendorf has described a most curious case of ten graduated forms of Planorbis multiformis in the successive beds of a fresh-water formation in Switzerland.

We shall, perhaps, best perceive the improbability of our being enabled to connect species by numerous fine, intermediate, fossil links by asking ourselves whether, for instance, geologists at some future period will be able to prove that our different breeds of cattle, sheep, horses, and dogs are descended from a single stock or from several aboriginal stocks; or, again, whether certain sea shells inhabiting the shores of North America, which are ranked by some conchologists as distinct species from their European representatives, and by other conchologists as only varieties, are really varieties, or are, as it is called, specifically distinct. This could be effected by the future geologist only by his discovering in a fossil state numerous intermediate gradations; and such success is improbable in the highest degree.

It has been asserted over and over again, by writers who believe in the immutability of species, that geology yields no linking forms. This assertion, as we shall see in the next chapter, is certainly erroneous. As Sir J. Lubbock has remarked, "Every species is a link between other allied forms." If we take a genus having a score of species, recent and extinct, and destroy four fifths of them, no one doubts that the remainder will stand much more distinct from each other. If the extreme forms in the genus happen to have been thus destroyed, the genus itself will stand more distinct from other allied genera. What geological research has not revealed is the former existence of infinitely numerous gradations, as fine as existing varieties, connecting together nearly all existing and extinct species. But this ought not to be expected; yet this has been repeatedly advanced as a most serious objection against my views.

On the sudden Appearance of whole Groups of allied Species

The abrupt manner in which whole groups of species suddenly appear in certain formations has been urged by several palæontologists—for instance, by Agassiz, Pictet, and Sedgwick—as a fatal objection to the be-

lief in the transmutation of species. If numerous species, belonging to the same genera or families, have really started into life at once, the fact would be fatal to the theory of evolution through natural selection. For the development by this means of a group of forms, all of which are descended from some one progenitor, must have been an extremely slow process; and the progenitors must have lived long before their modified descendants. But we continually overrate the perfection of the geological record, and falsely infer, because certain genera or families have not been found beneath a certain stage, that they did not exist before that stage. In all cases positive palæontological evidence may be implicitly trusted; negative evidence is worthless, as experience has so often shown. We continually forget how large the world is, compared with the area over which our geological formations have been carefully examined; we forget that groups of species may elsewhere have long existed, and have slowly multiplied, before they invaded the ancient archipelagoes of Europe and the United States. We do not make due allowance for the intervals of time which have elapsed between our consecutive formations—longer perhaps in many cases than the time required for the accumulation of each formation. These intervals will have given time for the multiplication of species from some one parent form: and in the succeeding formation, such groups or species will appear as if suddenly created.

I may recall the well-known fact that in geological treatises, published not many years ago, mammals were always spoken of as having abruptly come in at the commencement of the tertiary series. And now one of the richest known accumulations of fossil mammals belongs to the middle of the secondary series; and true mammals have been discovered in the new red sandstone at nearly the commencement of this great series. Cuvier used to urge that no monkey occurred in any tertiary stratum; but now extinct species have been discovered in India, South America, and in Europe, as far back as the miocene stage. Had it not been for the rare accident of the preservation of the footsteps in the new red sandstone of the United States, who would have ventured to suppose that no less than at least thirty different bird-like animals, some of gigantic size, existed during that period? Not a fragment of bone has been discovered in these beds. Not long ago, palæontologists maintained that the whole class of birds came suddenly into existence during the eocene period; but now we know, on the authority of Professor Owen, that a bird certainly lived during the deposition of the upper greensand; and, still more recently, that strange bird, the Archeopteryx, with a long lizard-like tail, bearing a pair of feathers on each joint, and with its wings furnished with two free claws, has been discovered in the oolitic slates of Solenhofen. Hardly any recent discovery shows more forcibly than this how little we as yet know of the former inhabitants of the world.

From these considerations, from our ignorance of the geology of other countries beyond the confines of Europe and the United States, and from the revolution in our palæontological knowledge effected by the discoveries of the last dozen years, it seems to me to be about as rash to

dogmatize on the succession of organic forms throughout the world as it would be for a naturalist to land for five minutes on a barren point in Australia and then to discuss the number and range of its productions.

On the sudden Appearance of Groups of allied Species in the lowest known Fossiliferous Strata

There is another and allied difficulty, which is much more serious. I allude to the manner in which species belonging to several of the main divisions of the animal kingdom suddenly appear in the lowest known fossiliferous rocks. Most of the arguments which have convinced me that all the existing species of the same group are descended from a single progenitor apply with equal force to the earliest known species. For instance, it cannot be doubted that all the Cambrian and Silurian trilobites are descended from some one crustacean, which must have lived long before the Cambrian age, and which probably differed greatly from any known animal. Some of the most ancient animals, as the Nautilus, Lingula, &c., do not differ much from living species; and it cannot on our theory be supposed that these old species were the progenitors of all the species belonging to the same groups which have subsequently appeared, for they are not in any degree intermediate in character.

Consequently, if the theory be true, it is indisputable that before the lowest Cambrian stratum was deposited long periods elapsed, as long as, or probably far longer than, the whole interval from the Cambrian age to the present day; and that during these vast periods the world swarmed with living creatures.

To the question why we do not find rich fossiliferous deposits belonging to these assumed earliest periods prior to the Cambrian system, I can give no satisfactory answer. Several eminent geologists, with Sir R. Murchison at their head, were until recently convinced that we beheld in the organic remains of the lowest Silurian stratum the first dawn of life. Other highly competent judges, as Lyell and E. Forbes, have disputed this conclusion. We should not forget that only a small portion of the world is known with accuracy. Not very long ago M. Barrande added another and lower stage, abounding with new and peculiar species, beneath the then known Silurian system; and now, still lower down in the Lower Cambrian formation, Mr. Hicks has found in South Wales beds rich in trilobites and containing various molluscs and annelids. The presence of phosphatic nodules and bituminous matter, even in some of the lowest azoic rocks, probably indicates life at these periods; and the existence of the Eozoon in the Laurentian formation of Canada is generally admitted. There are three great series of strata beneath the Silurian system in Canada, in the lowest of which the Eozoon is found. Sir W. Logan states that their "united thickness may possibly far surpass that of all the succeeding rocks, from the base of the palæozoic series to the present time. We are thus carried back to a period so remote that the appearance of the so-

called Primordial fauna (of Barrande) may by some be considered as a comparatively modern event." The Eozoon belongs to the most lowly organised of all classes of animals, but is highly organised for its class; it existed in countless numbers, and, as Dr. Dawson has remarked, certainly preyed on other minute organic beings, which must have lived in great numbers.

The several difficulties here discussed, namely—that, though we find in our geological formations many links between the species which now exist and which formerly existed, we do not find infinitely numerous fine transitional forms closely joining them all together;—the sudden manner in which several groups of species first appear in our European formations;—the almost entire absence, as at present known, of formations rich in fossils beneath the Cambrian strata—are all undoubtedly of the most serious nature. We see this in the fact that the most eminent palæontologists, namely, Cuvier, Agassiz, Barrande, Pictet, Falconer, E. Forbes, &c., and all our greatest geologists, as Lyell, Murchison, Sedgwick, &c., have unanimously, often vehemently, maintained the immutability of species. But Sir Charles Lyell now gives the support of his high authority to the opposite side; and most geologists and palæontologists are much shaken in their former belief. Those who believe that the geological record is in any degree perfect will undoubtedly at once reject the theory. For my part, following out Lyell's metaphor, I look at the geological record as a history of the world imperfectly kept, and written in a changing dialect; of this history we possess the last volume alone, relating only to two or three countries. Of this volume, only here and there a short chapter has been preserved; and of each page, only here and there a few lines. Each word of the slowly changing language, more or less different in the successive chapters, may represent the forms of life, which are entombed in our consecutive formations, and which falsely appear to have been abruptly introduced. On this view, the difficulties above discussed are greatly diminished, or even disappear.

X. ON THE GEOLOGICAL SUCCESSION OF ORGANIC BEINGS

Let us now see whether the several facts and laws relating to the geological succession of organic beings accord best with the common view of the immutability of species or with that of their slow and gradual modification, through variation and natural selection.

New species have appeared very slowly, one after another, both on the land and in the waters. Lyell has shown that it is hardly possible to resist the evidence on this head.

Species belonging to different genera and classes have not changed at the same rate, or in the same degree. In the older tertiary beds a few living shells may still be found in the midst of a multitude of extinct forms. Falconer has given a striking instance of a similar fact, for an ex-

isting crocodile is associated with many lost mammals and reptiles in the sub-Himalayan deposits. Yet if we compare any but the most closely related formations, all the species will be found to have undergone some change. When a species has once disappeared from the face of the earth, we have no reason to believe that the same identical form ever reappears.

These several facts accord well with our theory, which includes no fixed law of development, causing all the inhabitants of an area to change abruptly, or simultaneously, or to an equal degree. The process of modification must be slow, and will generally affect only a few species at the same time; for the variability of each species is independent of that of all others. Whether such variations or individual differences as may arise will be accumulated through natural selection in a greater or less degree, thus causing a greater or less amount of permanent modification, will depend on many complex contingencies—on the variations being of a beneficial nature, on the freedom of intercrossing, on the slowly changing physical conditions of the country, on the immigration of new colonists, and on the nature of the other inhabitants with which the varying species come into competition. Hence it is by no means surprising that one species should retain the same identical form much longer than others; or, if changing, should change in a less degree. When many of the inhabitants of any area have become modified and improved, we can understand, on the principle of competition, and from the all-important relations of organism to organism in the struggle for life, that any form which did not become in some degree modified and improved would be liable to extermination. Hence we see why all the species in the same region do at last, if we look to long enough intervals of time, become modified, for otherwise they would become extinct.

On Extinction

We have as yet only spoken incidentally of the disappearance of species and of groups of species. On the theory of natural selection, the extinction of old forms and the production of new and improved forms are intimately connected together. The old notion of all the inhabitants of the earth having been swept away by catastrophes at successive periods is very generally given up, even by those geologists, as Elie de Beaumont, Murchison, Barrande, &c., whose general views would naturally lead them to this conclusion. On the contrary, we have every reason to believe, from the study of the tertiary formations, that species and groups of species gradually disappear, one after another, first from one spot, then from another, and finally from the world. In some few cases, however, as by the breaking of an isthmus and the consequent irruption of a multitude of new inhabitants into an adjoining sea, or by the final subsidence of an island, the process of extinction may have been rapid. Both single species and whole groups of species last for very unequal periods; some groups, as we have seen, have endured from the earliest known dawn of life to

the present day; some have disappeared before the close of the palæozoic period. No fixed law seems to determine the length of time during which any single species or any single genus endures. There is reason to believe that the extinction of a whole group of species is generally a slower process than their production: if their appearance and disappearance be represented by a vertical line of varying thickness, the line is found to taper more gradually at its upper end, which marks the progress of extermination, than at its lower end, which marks the first appearance and the early increase in number of the species.

The theory of natural selection is grounded on the belief that each new variety, and ultimately each new species, is produced and maintained by having some advantage over those with which it comes into competition; and the consequent extinction of the less-favoured forms almost inevitably follows. It is the same with our domestic productions; when a new and slightly improved variety has been raised, it at first supplants the less improved varieties in the same neighbourhood; when much improved it is transported far and near, like our Shorthorn cattle, and takes the place of other breeds in other countries. Thus the appearance of new forms and the disappearance of old forms, both those naturally and those artificially produced, are bound together. In flourishing groups, the number of new specific forms which have been produced within a given time has at some periods probably been greater than the number of the old specific forms which have been exterminated; but we know that species have not gone on indefinitely increasing, at least during the later geological epochs, so that, looking to later times, we may believe that the production of new forms has caused the extinction of about the same number of old forms.

Thus, as it seems to me, the manner in which single species and whole groups of species become extinct accords well with the theory of natural selection. We need not marvel at extinction; if we must marvel, let it be at our own presumption in imagining for a moment that we understand the many complex contingencies on which the existence of each species depends. If we forget for an instant that each species tends to increase inordinately, and that some check is always in action, yet seldom perceived by us, the whole economy of nature will be utterly obscured. Whenever we can precisely say why this species is more abundant in individuals than that; why this species and not another can be naturalised in a given country; then, and not until then, we may justly feel surprise why we cannot account for the extinction of any particular species or group of species.

On the Forms of Life changing almost simultaneously throughout the World

Scarcely any palæontological discovery is more striking than the fact that the forms of life change almost simultaneously throughout the world,

Thus our European Chalk formation can be recognised in many distant regions, under the most different climates, where not a fragment of the mineral chalk itself can be found; namely in North America, in equatorial South America, in Tierra del Fuego, at the Cape of Good Hope, and in the peninsula of India. For at these distant points, the organic remains in certain beds present an unmistakable resemblance to those of the Chalk. It is not that the same species are met with; for in some cases not one species is identically the same, but they belong to the same families, genera, and sections of genera, and sometimes are similarly characterised in such trifling points as mere superficial sculpture. Moreover, other forms, which are not found in the Chalk of Europe, but which occur in the formations either above or below, occur in the same order at these distant points of the world. In the several successive palæozoic formations of Russia, Western Europe, and North America, a similar parallelism in the forms of life has been observed by several authors; so it is, according to Lyell, with the European and North American tertiary deposits.

This great fact of the parallel succession of the forms of life throughout the world is explicable on the theory of natural selection. New species are formed by having some advantage over older forms; and the forms, which are already dominant, or have some advantage over the other forms in their own country, give birth to the greatest number of new varieties or incipient species. We have distinct evidence on this head, in the plants which are dominant, that is, which are commonest and most widely diffused, producing the greatest number of new varieties. It is also natural that the dominant, varying, and far-spreading species, which have already invaded to a certain extent the territories of other species, should be those which would have the best chance of spreading still further, and of giving rise in new countries to other new varieties and species. The process of diffusion would often be very slow, depending on climatal and geographical changes, on strange accidents, and on the gradual acclimatisation of new species to the various climates through which they might have to pass, but in the course of time the dominant forms would generally succeed in spreading and would ultimately prevail. The diffusion would, it is probable, be slower with the terrestrial inhabitants of distinct continents than with the marine inhabitants of the continuous sea. We might therefore expect to find, as we do find, a less strict degree of parallelism in the succession of the productions of the land than with those of the sea.

On the Affinities of Extinct Species to each other, and to Living Forms

Let us now look to the mutual affinities of extinct and living species. All fall into a few grand classes; and this fact is at once explained on the principle of descent. The more ancient any form is, the more, as a general rule, it differs from living forms. But, as Buckland long ago remarked, extinct species can all be classed either in still existing groups or between them. That the extinct forms of life help to fill up the intervals between

existing genera, families, and orders is certainly true. If we confine our attention either to the living or to the extinct species of the same class, the series is far less perfect than if we combine both into one general system.

Cuvier ranked the Ruminants and Pachyderms as two of the most distinct orders of mammals: but so many fossil links have been disentombed that Owen has had to alter the whole classification, and has placed certain pachyderms in the same sub-order with ruminants; for example, he dissolves by gradations the apparently wide interval between the pig and the camel. The Ungulata or hoofed quadrupeds are now divided into the even-toed or odd-toed divisions; but the Macrauchenia of South America connects to a certain extent these two grand divisions. No one will deny that the Hipparion is intermediate between the existing horse and certain older ungulate forms.

Let us see how far these several facts and inferences accord with the theory of descent with modification. As the subject is somewhat complex, I must request the reader to turn to the diagram in the fourth chapter. We may suppose that the numbered letters in italics represent genera; and the dotted lines diverging from them the species in each genus. The diagram is much too simple, too few genera and too few species being given, but this is unimportant for us. The horizontal lines may represent successive geological formations, and all the forms beneath the uppermost line may be considered as extinct. The three existing genera, a^{14}, q^{14}, p^{14}, will form a small family; b^{14} and f^{14} a closely allied family or sub-family; and o^{14}, e^{14}, m^{14}, a third family. These three families, together with the many extinct genera on the several lines of descent diverging from the parent form (A) will form an order, for all will have inherited something in common from their ancient progenitor. On the principle of the continued tendency to divergence of character, which was formerly illustrated by this diagram, the more recent any form is, the more it will generally differ from its ancient progenitor. Hence we can understand the rule that the most ancient fossils differ most from existing forms. We must not, however, assume that divergence of character is a necessary contingency; it depends solely on the descendants from a species being thus enabled to seize on many and different places in the economy of nature. Therefore it is quite possible, as we have seen in the case of some Silurian forms, that a species might go on being slightly modified in relation to its slightly altered conditions of life, and yet retain throughout a vast period the same general characteristics. This is represented in the diagram by the letter r^{14}.

All the many forms, extinct and recent, descended from (A) make, as before remarked, one order; and this order, from the continued effects of extinction and divergence of character, has become divided into several sub-families and families, some of which are supposed to have perished at different periods, and some to have endured to the present day.

By looking at the diagram we can see that if many of the extinct forms supposed to be embedded in the successive formations were dis-

covered at several points low down in the series, the three existing families on the uppermost line would be rendered less distinct from each other. If, for instance, the genera a^1, a^5, a^{10}, f^8, m^3, m^6, m^9 were disinterred, these three families would be so closely linked together that they probably would have to be united into one great family, in nearly the same manner as has occurred with ruminants and certain pachyderms. Yet he who objected to consider as intermediate the extinct genera, which thus link together the living genera of three families, would be partly justified, for they are intermediate, not directly, but only by a long and circuitous course through many widely different forms.

Under nature the process will be far more complicated than is represented in the diagram; for the groups will have been more numerous; they will have endured for extremely unequal lengths of time, and will have been modified in various degrees. As we possess only the last volume of the geological record, and that in a very broken condition, we have no right to expect, except in rare cases, to fill up the wide intervals in the natural system, and thus to unite distinct families or orders. All that we have a right to expect is that those groups which have, within known geological periods, undergone much modification should in the older formations make some slight approach to each other; so that the older members should differ less from each other in some of their characters than do the existing members of the same groups; and this, by the concurrent evidence of our best palæontologists, is frequently the case.

Thus, on the theory of descent with modification, the main facts with respect to the mutual affinities of the extinct forms of life to each other and to living forms are explained in a satisfactory manner. And they are wholly inexplicable on any other view.

On the Succession of the same Types within the same Areas, during the later Tertiary Periods

Mr. Clift many years ago showed that the fossil mammals from the Australian caves were closely allied to the living marsupials of that continent. In South America a similar relationship is manifest, even to an uneducated eye, in the gigantic pieces of armour, like those of the armadillo, found in several parts of La Plata; and Professor Owen has shown in the most striking manner that most of the fossil mammals, buried there in such numbers, are related to South American types.

Now what does this remarkable law of the succession of the same types within the same areas mean? He would be a bold man who, after comparing the present climate of Australia and of parts of South America, under the same latitude, would attempt to account, on the one hand through dissimilar physical conditions, for the dissimilarity of the inhabitants of these two continents; and, on the other hand, through similarity of conditions, for the uniformity of the same types in each continent during the later tertiary periods. Nor can it be pretended that it is an

immutable law that marsupials should have been chiefly or solely produced in Australia; or that Edentata and other American types should have been solely produced in South America. For we know that Europe in ancient times was peopled by numerous marsupials.

On the theory of descent with modification, the great law of the long enduring, but not immutable, succession of the same types within the same areas is at once explained; for the inhabitants of each quarter of the world will obviously tend to leave in that quarter, during the next succeeding period of time, closely allied though in some degree modified descendants. If the inhabitants of one continent formerly differed greatly from those of another continent, so will their modified descendants still differ in nearly the same manner and degree. But after very long intervals of time, and after great geographical changes, permitting much intermigration, the feebler will yield to the more dominant forms, and there will be nothing immutable in the distribution of organic beings.

XI. GEOGRAPHICAL DISTRIBUTION

In considering the distribution of organic beings over the face of the globe, the first great fact which strikes us is that neither the similarity nor the dissimilarity of the inhabitants of various regions can be wholly accounted for by climatal and other physical conditions. Of late, almost every author who has studied the subject has come to this conclusion. The case of America alone would almost suffice to prove its truth; for if we exclude the arctic and northern temperate parts, all authors agree that one of the most fundamental divisions in geographical distribution is that between the New and Old Worlds; yet if we travel over the vast American continent, from the central parts of the United States to its extreme southern point, we meet with the most diversified conditions: humid districts, arid deserts, lofty mountains, grassy plains, forests, marshes, lakes, and great rivers, under almost every temperature. There is hardly a climate or condition in the Old World which cannot be paralleled in the New—at least as closely as the same species generally require. No doubt small areas can be pointed out in the Old World hotter than any in the New World; but these are not inhabited by a fauna different from that of the surrounding districts; for it is rare to find a group of organisms confined to a small area, of which the conditions are peculiar in only a slight degree. Notwithstanding this general parallelism in the conditions of the Old and New Worlds, how widely different are their living productions!

A second great fact which strikes us in our general review is that barriers of any kind, or obstacles to free migration, are related in a close and important manner to the differences between the productions of various regions. We see this in the great difference in nearly all the terrestrial productions of the New and Old Worlds, excepting in the northern parts, where the land almost joins, and where, under a slightly different climate, there might have been free migration for the northern temperate forms,

as there now is for the strictly arctic productions. We see the same fact in the great difference between the inhabitants of Australia, Africa, and South America under the same latitude; for these countries are almost as much isolated from each other as is possible. On each continent, also, we see the same fact; for on the opposite sides of lofty and continuous mountain ranges, of great deserts and even of large rivers, we find different productions; though as mountain chains, deserts, &c., are not as impassable, or likely to have endured so long, as the oceans separating continents, the differences are very inferior in degree to those characteristic of distinct continents.

A third great fact, partly included in the foregoing statement, is the affinity of the productions of the same continent or of the same sea, though the species themselves are distinct at different points and stations. It is a law of the widest generality, and every continent offers innumerable instances. Nevertheless the naturalist, in travelling, for instance, from north to south, never fails to be struck by the manner in which successive groups of beings, specifically distinct, though nearly related, replace each other. He hears from closely allied, yet distinct kinds of birds notes nearly similar, and sees their nests similarly constructed, but not quite alike, with eggs coloured in nearly the same manner. The plains near the Straits of Magellan are inhabited by one species of Rhea (American ostrich), and northward the plains of La Plata by another species of the same genus; and not by a true ostrich or emu, like those inhabiting Africa and Australia under the same latitude. On these same plains of La Plata we see the agouti and bizcacha, animals having nearly the same habits as our hares and rabbits, and belonging to the same order of Rodents, but they plainly display an American type of structure. We ascend the lofty peaks of the Cordillera, and we find an alpine species of bizcacha; we look to the waters, and we do not find the beaver or muskrat, but the coypu and capybara, rodents of the South American type. Innumerable other instances could be given. If we look to the islands off the American shore, however much they may differ in geological structure, the inhabitants are essentially American, though they may be all peculiar species. We may look back to past ages, as shown in the last chapter, and we find American types then prevailing on the American continent and in the American seas. We see in these facts some deep organic bond, throughout space and time, over the same areas of land and water, independently of physical conditions. The naturalist must be dull who is not led to enquire what this bond is.

The bond is simply inheritance, that cause which alone, as far as we positively know, produces organisms quite like each other, or, as we see in the case of varieties, nearly alike. The dissimilarity of the inhabitants of different regions may be attributed to modification through variation and natural selection, and probably in a subordinate degree to the definite influence of different physical conditions. The degrees of dissimilarity will depend on the migration of the more dominant forms of life from one region into another having been more or less effectually prevented, at peri-

ods more or less remote;—on the nature and number of the former immigrants;—and on the action of the inhabitants on each other in leading to the preservation of different modifications; the relation of organism to organism in the struggle for life being, as I have already often remarked, the most important of all relations. Thus the high importance of barriers comes into play by checking migration; as does time for the slow process of modification through natural selection.

According to these views, it is obvious that the several species of the same genus, though inhabiting the most distant quarters of the world, must originally have proceeded from the same source, as they are descended from the same progenitor. In the case of those species which have undergone during the whole geological periods little modification, there is not much difficulty in believing that they have migrated from the same region; for during the vast geographical and climatal changes which have supervened since ancient times, almost any amount of migration is possible. But in many other cases, in which we have reason to believe that the species of a genus have been produced within comparatively recent times, there is great difficulty on this head. It is also obvious that the individuals of the same species, though now inhabiting distant and isolated regions, must have proceeded from one spot, where their parents were first produced: for, as has been explained, it is incredible that individuals identically the same should have been produced from parents specifically distinct.

Single Centres of supposed Creation.—We are thus brought to the question which has been largely discussed by naturalists, namely, whether species have been created at one or more points of the earth's surface. Undoubtedly there are many cases of extreme difficulty in understanding how the same species could possibly have migrated from some one point to the several distant and isolated points, where now found. Nevertheless the simplicity of the view that each species was first produced within a single region captivates the mind. He who rejects it rejects the *vera causa* of ordinary generation with subsequent migration, and calls in the agency of a miracle. It is universally admitted that in most cases the area inhabited by a species is continuous; and that when a plant or animal inhabits two points so distant from each other, or with an interval of such a nature that the space could not have been easily passed over by migration, the fact is given as something remarkable and exceptional. The incapacity of migrating across a wide sea is more clear in the case of terrestrial mammals than perhaps with any other organic beings; and, accordingly, we find no inexplicable instances of the same mammals inhabiting distant points of the world.

Hence it seems to me, as it has to many other naturalists, that the view of each species having been produced in one area alone, and having subsequently migrated from that area as far as its powers of migration and subsistence under past and present conditions permitted, is the most probable. Undoubtedly many cases occur, in which we cannot explain how the same species could have passed from one point to the other. But the

geographical and climatal changes which have certainly occurred within recent geological times must have rendered discontinuous the formerly continuous range of many species. So that we are reduced to consider whether the exceptions to continuity of range are so numerous and of so grave a nature that we ought to give up the belief, rendered probable by general considerations, that each species has been produced within one area, and has migrated thence as far as it could. If the existence of the same species at distant and isolated points of the earth's surface can in many instances be explained on the view of each species having migrated from a single birthplace; then, considering our ignorance with respect to former climatal and geographical changes and to the various occasional means of transport, the belief that a single birthplace is the law seems to me incomparably the safest.

In botanical works, this or that plant is often stated to be ill adapted for wide dissemination; but the greater or less facilities for transport across the sea may be said to be almost wholly unknown. Until I tried, with Mr. Berkeley's aid, a few experiments, it was not even known how far seeds could resist the injurious action of sea water. To my surprise I found that out of 87 kinds, 64 germinated after an immersion of 28 days and a few survived an immersion of 137 days. It is well known what a difference there is in the buoyancy of green and seasoned timber; and it occurred to me that floods would often wash into the sea dried plants or branches with seed capsules or fruit attached to them. Hence I was led to dry the stems and branches of 94 plants with ripe fruit, and to place them on sea water. The majority sank rapidly, but some which, whilst green, floated for a short time, when dried floated much longer; for instance, ripe hazelnuts sank immediately, but when dried they floated for 90 days, and afterwards, when planted, germinated; an asparagus plant with ripe berries floated for 23 days, when dried it floated for 85 days, and the seeds afterwards germinated; the ripe seeds of Heloscíadium sank in two days, when dried they floated for above 90 days, and afterwards germinated. Altogether, out of the 94 dried plants, 18 floated for above 28 days; and some of the 18 floated for a very much longer period. So that as $^{64}/_{87}$ kinds of seeds germinated after an immersion of 28 days; and as $^{18}/_{94}$ distinct species with ripe fruit (but not all the same species as in the foregoing experiment) floated, after being dried, for above 28 days, we may conclude, as far as anything can be inferred from these scanty facts, that the seeds of $^{14}/_{100}$ kinds of plants of any country might be floated by sea currents during 28 days and would retain their power of germination. In Johnston's *Physical Atlas,* the average rate of the several Atlantic currents is 33 miles per diem (some currents running at the rate of 60 miles per diem); on this average, the seeds of $^{14}/_{100}$ plants belonging to one country might be floated across 924 miles of sea to another country, and when stranded, if blown by an inland gale to a favourable spot, would germinate.

Living birds can hardly fail to be highly effective agents in the transportation of seeds. I could give many facts showing how frequently birds

of many kinds are blown by gales to vast distances across the ocean. We may safely assume that under such circumstances their rate of flight would often be 35 miles an hour; and some authors have given a far higher estimate. I have never seen an instance of nutritious seeds passing through the intestines of a bird; but hard seeds of fruit pass uninjured through even the digestive organs of a turkey. In the course of two months, I picked up in my garden 12 kinds of seeds, out of the excrement of small birds, and these seemed perfect, and some of them, which were tried, germinated. But the following fact is more important: the crops of birds do not secrete gastric juice, and do not, as I know by trial, injure in the least the germination of seeds; now, after a bird has found and devoured a large supply of food, it is positively asserted that all the grains do not pass into the gizzard for twelve or even eighteen hours. A bird in this interval might easily be blown to the distance of 500 miles, and hawks are known to look out for tired birds, and the contents of their torn crops might thus readily get scattered.

Although the beaks and feet of birds are generally clean, earth sometimes adheres to them: in one case I removed sixty-one grains, and in another case twenty-two grains of dry argillaceous earth from the foot of a partridge, and in the earth there was a pebble as large as the seed of a vetch.

Considering that these several means of transport, and that other means, which without doubt remain to be discovered, have been in action year after year for tens of thousands of years, it would, I think, be a marvellous fact if many plants had not thus become widely transported. It should be observed that scarcely any means of transport would carry seeds for very great distances: for seeds do not retain their vitality when exposed for a great length of time to the action of sea water; nor could they be long carried in the crops or intestines of birds. These means, however, would suffice for occasional transport across tracts of sea some hundred miles in breadth, or from island to island, or from a continent to a neighbouring island, but not from one distant continent to another. The floras of distant continents would not by such means become mingled; but would remain as distinct as they now are. The currents, from their course, would never bring seeds from North America to Britain, though they might and do bring seeds from the West Indies to our western shores, where, if not killed by their very long immersion in salt water, they could not endure our climate.

But it would be a great error to argue that because a well-stocked island, like Great Britain, has not, as far as is known (and it would be very difficult to prove this), received within the last few centuries, through occasional means of transport, immigrants from Europe or any other continent, that a poorly stocked island, though standing more remote from the mainland, would not receive colonists by similar means. Out of a hundred kinds of seeds or animals transported to an island, even if far less well-stocked than Britain, perhaps not more than one would be so well fitted to its new home as to become naturalised. But this is no

valid argument against what would be effected by occasional means of transport, during the long lapse of geological time, whilst the island was being upheaved, and before it had become fully stocked with inhabitants. On almost bare land, with few or no destructive insects or birds living there, nearly every seed which chanced to arrive, if fitted for the climate, would germinate and survive.

Dispersal during the Glacial Period

The identity of many plants and animals, on mountain summits, separated from each other by hundreds of miles of lowlands, where Alpine species could not possibly exist, is one of the most striking cases known of the same species living at distant points without the apparent possibility of their having migrated from one point to the other. It is indeed a remarkable fact to see so many plants of the same species living on the snowy regions of the Alps or Pyrenees and in the extreme northern parts of Europe; but it is far more remarkable that the plants on the White Mountains, in the United States of America, are all the same with those of Labrador, and nearly all the same, as we hear from Asa Gray, with those on the loftiest mountains of Europe. Even as long ago as 1747, such facts led Gmelin to conclude that the same species must have been independently created at many distinct points; and we might have remained in this same belief had not Agassiz and others called vivid attention to the Glacial period, which, as we shall immediately see, affords a simple explanation of these facts. We have evidence of almost every conceivable kind, organic and inorganic, that, within a very recent geological period, central Europe and North America suffered under an arctic climate. The ruins of a house burnt by fire do not tell their tale more plainly than do the mountains of Scotland and Wales, with their scored flanks, polished surfaces, and perched boulders, of the icy streams with which their valleys were lately filled. So greatly has the climate of Europe changed that in Northern Italy gigantic moraines, left by old glaciers, are now clothed by the vine and maize. Throughout a large part of the United States, erratic boulders and scored rocks plainly reveal a former cold period.

The former influence of the glacial climate on the distribution of the inhabitants of Europe, as explained by Edward Forbes, is substantially as follows. But we shall follow the changes more readily, by supposing a new Glacial period slowly to come on, and then pass away, as formerly occurred. As the cold came on, and as each more southern zone became fitted for the inhabitants of the north, these would take the places of the former inhabitants of the temperate regions. The latter, at the same time, would travel further and further southward, unless they were stopped by barriers, in which case they would perish. The mountains would become covered with snow and ice, and their former Alpine inhabitants would descend to the plains. By the time that the cold had reached its maximum, we should have an arctic fauna and flora, covering the central parts of

Europe, as far south as the Alps and Pyrenees, and even stretching into Spain. The now temperate regions of the United States would likewise be covered by arctic plants and animals, and these would be nearly the same with those of Europe; for the present circumpolar inhabitants, which we suppose to have everywhere travelled southward, are remarkably uniform round the world.

As the warmth returned, the arctic forms would retreat northward, closely followed up in their retreat by the productions of the more temperate regions. And as the snow melted from the bases of the mountains, the arctic forms would seize on the cleared and thawed ground, always ascending, as the warmth increased and the snow still further disappeared, higher and higher, whilst their brethren were pursuing their northern journey. Hence, when the warmth had fully returned, the same species, which had lately lived together on the European and North American lowlands, would again be found in the arctic regions of the Old and New Worlds, and on many isolated mountain summits far distant from each other.

Thus we can understand the identity of many plants at points so immensely remote as the mountains of the United States and those of Europe. We can thus also understand the fact that the Alpine plants of each mountain range are more especially related to the arctic forms living due north or nearly due north of them: for the first migration when the cold came on, and the remigration on the returning warmth, would generally have been due south and north. The Alpine plants, for example, of Scotland, as remarked by Mr. H. C. Watson, and those of the Pyrenees, as remarked by Raymond, are more especially allied to the plants of Northern Scandinavia; those of the United States to Labrador; those of the mountains of Siberia to the arctic regions of that country. These views, grounded as they are on the perfectly well-ascertained occurrence of a former Glacial period, seem to me to explain in so satisfactory a manner the present distribution of the Alpine and Arctic productions of Europe and America that when in other regions we find the same species on distant mountain summits, we may almost conclude, without other evidence, that a colder climate formerly permitted their migration across the intervening lowlands, now become too warm for their existence.

XII. GEOGRAPHICAL DISTRIBUTION—Continued

Fresh-water Productions

As LAKES and river systems are separated from each other by barriers of land, it might have been thought that fresh-water productions would not have ranged widely within the same country, and as the sea is apparently a still more formidable barrier, that they would never have extended to distant countries. But the case is exactly the reverse. Not only have many fresh-water species, belonging to different classes, an enormous range, but

allied species prevail in a remarkable manner throughout the world. When first collecting in the fresh waters of Brazil, I well remember feeling much surprise at the similarity of the fresh-water insects, shells, &c., and at the dissimilarity of the surrounding terrestrial beings compared with those of Britain.

But the wide ranging power of fresh-water productions can, I think, in most cases be explained by their having become fitted, in a manner highly useful to them, for short and frequent migrations from pond to pond, or from stream to stream, within their own countries; and liability to wide dispersal would follow from this capacity as an almost necessary consequence. We can here consider only a few cases; of these, some of the most difficult to explain are presented by fish. It was formerly believed that the same fresh-water species never existed on two continents distant from each other. But Dr. Günther has lately shown that the Galaxias attenuatus inhabits Tasmania, New Zealand, the Falkland Islands, and the mainland of South America. This is a wonderful case, and probably indicates dispersal from an Antarctic centre during a former warm period. This case, however, is rendered in some degree less surprising by the species of this genus having the power of crossing by some unknown means considerable spaces of open ocean: thus there is one species common to New Zealand and to the Auckland Islands, though separated by a distance of about 230 miles. On the same continent fresh-water fish often range widely, and as if capriciously; for in two adjoining river systems some of the species may be the same and some wholly different.

It is probable that they are occasionally transported by what may be called accidental means. Thus fishes still alive are not very rarely dropped at distant points by whirlwinds; and it is known that the ova retain their vitality for a considerable time after removal from the water. Their dispersal may, however, be mainly attributed to changes in the level of the land within the recent period, causing rivers to flow into each other. Instances, also, could be given of this having occurred during floods, without any change of level.

With respect to plants, it has long been known what enormous ranges many fresh-water and even marsh species have, both over continents and to the most remote oceanic islands. This is strikingly illustrated, according to Alphonse de Candolle, in those large groups of terrestrial plants which have very few aquatic members; for the latter seem immediately to acquire, as if in consequence, a wide range. I think favourable means of dispersal explain this fact. I have before mentioned that earth occasionally adheres in some quantity to the feet and beaks of birds. Wading birds, which frequent the muddy edges of ponds, if suddenly flushed, would be the most likely to have muddy feet. Birds of this order wander more than those of any other; and they are occasionally found on the most remote and barren islands of the open ocean; they would not be likely to alight on the surface of the sea, so that any dirt on their feet would not be washed off; and when gaining the land, they would be sure to fly to their natural fresh-water haunts.

The species of all kinds which inhabit oceanic islands are few in number compared with those on equal continental areas: Alphonse de Candolle admits this for plants, and Wollaston for insects. New Zealand, for instance, with its lofty mountains and diversified stations, extending over 780 miles of latitude, together with the outlying islands of Auckland, Campbell, and Chatham, contain altogether only 960 kinds of flowering plants; if we compare this moderate number with the species which swarm over equal areas in Southwestern Australia or at the Cape of Good Hope, we must admit that some cause, independently of different physical conditions, has given rise to so great a difference in number. Even the uniform county of Cambridge has 847 plants, and the little island of Anglesea 764, but a few ferns and a few introduced plants are included in these numbers, and the comparison in some other respects is not quite fair. We have evidence that the barren island of Ascension aboriginally possessed less than half-a-dozen flowering plants; yet many species have now become naturalised on it, as they have in New Zealand and on every other oceanic island which can be named. In St. Helena there is reason to believe that the naturalised plants and animals have nearly or quite exterminated many native productions. He who admits the doctrine of the creation of each separate species will have to admit that a sufficient number of the best adapted plants and animals were not created for oceanic islands; for man has unintentionally stocked them far more fully and perfectly than did nature.

Although in oceanic islands the species are few in number, the proportion of endemic kinds (*i. e.*, those found nowhere else in the world) is often extremely large. If we compare, for instance, the number of endemic land shells in Madeira, or of endemic birds in the Galapagos Archipelago, with the number found on any continent, and then compare the area of the island with that of the continent, we shall see that this is true. This fact might have been theoretically expected, for, as already explained, species occasionally arriving after long intervals of time in the new and isolated district, and having to compete with new associates, would be eminently liable to modification, and would often produce groups of modified descendants. But it by no means follows that, because in an island nearly all the species of one class are peculiar, those of another class, or of another section of the same class, are peculiar; and this difference seems to depend partly on the species which are not modified having immigrated in a body, so that their mutual relations have not been much disturbed; and partly on the frequent arrival of unmodified immigrants from the mother country, with which the insular forms have intercrossed. It should be borne in mind that the offspring of such crosses would certainly gain in vigour; so that even an occasional cross would produce more effect than might have been anticipated. I will give a few illustrations of the foregoing remarks: in the Galapagos Islands there are 26 land birds; of these 21 (or perhaps 23) are peculiar, whereas of the 11 marine birds only 2 are peculiar; and it is obvious that marine birds could arrive at these islands much more easily and frequently than land birds.

Bermuda, on the other hand, which lies at about the same distance from North America as the Galapagos Islands do from South America, and which has a very peculiar soil, does not possess a single endemic land bird, and we know from Mr. J. M. Jones's admirable account of Bermuda that very many North American birds occasionally or even frequently visit this island. Almost every year, as I am informed by Mr. E. V. Harcourt, many European and African birds are blown to Madeira; this island is inhabited by 99 kinds, of which one alone is peculiar, though very closely related to a European form; and three or four other species are confined to this island and to the Canaries. So that the islands of Bermuda and Madeira have been stocked from the neighbouring continents with birds, which for long ages have there struggled together and have become mutually coadapted. Hence, when settled in their new homes, each kind will have been kept by the others to its proper place and habits, and will consequently have been but little liable to modification. Any tendency to modification will also have been checked by intercrossing with the unmodified immigrants, often arriving from the mother country.

Absence of Batrachians and Terrestrial Mammals on Oceanic Islands

With respect to the absence of whole orders of animals on oceanic islands, Bory St. Vincent long ago remarked that Batrachians (frogs, toads, newts) are never found on any of the many islands with which the great oceans are studded. I have taken pains to verify this assertion, and have found it true, with the exception of New Zealand, New Caledonia, the Andaman Islands, and perhaps the Solomon Islands and the Seychelles. But it is doubtful whether New Zealand and New Caledonia ought to be classed as oceanic islands; and this is still more doubtful with respect to the Andaman and Solomon groups and the Seychelles. This general absence of frogs, toads, and newts on so many true oceanic islands cannot be accounted for by their physical conditions: indeed it seems that islands are peculiarly fitted for these animals; for frogs have been introduced into Madeira, the Azores, and Mauritius, and have multiplied so as to become a nuisance. But as these animals and their spawn are immediately killed (with the exception, as far as known, of one Indian species) by sea water, there would be great difficulty in their transportal across the sea, and therefore we can see why they do not exist on strictly oceanic islands. But why, on the theory of creation, they should not have been created there, it would be very difficult to explain.

Mammals offer another and similar case. I have carefully searched the oldest voyages and have not found a single instance, free from doubt, of a terrestrial mammal (excluding domesticated animals kept by the natives) inhabiting an island situated above 300 miles from a continent or great continental island; and many islands situated at a much less distance are equally barren. The Falkland Islands, which are inhabited by

a wolf-like fox, come nearest to an exception; but this group cannot be considered as oceanic, as it lies on a bank in connection with the mainland at the distance of about 280 miles; moreover, icebergs formerly brought boulders to its western shores, and they may have formerly transported foxes, as now frequently happens in the arctic regions. Yet it cannot be said that small islands will not support at least small mammals, for they occur in many parts of the world on very small islands, when lying close to a continent; and hardly an island can be named on which our smaller quadrupeds have not become naturalised and greatly multiplied.

On the Relations of the Inhabitants of Islands to those of the nearest Mainland

The most striking and important fact for us is the affinity of the species which inhabit islands to those of the nearest mainland, without being actually the same. Numerous instances could be given. The Galapagos Archipelago, situated under the equator, lies at the distance of between 500 and 600 miles from the shores of South America. Here almost every product of the land and of the water bears the unmistakable stamp of the American continent. There are twenty-six land birds; of these, twenty-one, or perhaps twenty-three, are ranked as distinct species, and would commonly be assumed to have been here created; yet the close affinity of most of these birds to American species is manifest in every character, in their habits, gestures, and tones of voice. So it is with the other animals, and with a large proportion of the plants, as shown by Dr. Hooker in his admirable Flora of this archipelago. The naturalist, looking at the inhabitants of these volcanic islands in the Pacific, distant several hundred miles from the continent, feels that he is standing on American land. Why should this be so? Why should the species which are supposed to have been created in the Galapagos Archipelago, and nowhere else, bear so plainly the stamp of affinity to those created in America? There is nothing in the conditions of life, in the geological nature of the islands, in their height or climate, or in the proportions in which the several classes are associated together, which closely resembles the conditions of the South American coast: in fact, there is a considerable dissimilarity in all these respects. On the other hand, there is a considerable degree of resemblance in the volcanic nature of the soil, in the climate, height, and size of the islands, between the Galapagos and Cape Verde Archipelagoes: but what an entire and absolute difference in their inhabitants! The inhabitants of the Cape Verde Islands are related to those of Africa, like those of the Galapagos to America. Facts such as these admit of no sort of explanation on the ordinary view of independent creation; whereas on the view here maintained, it is obvious that the Galapagos Islands would be likely to receive colonists from America, whether by occasional means of transport or (though I do not believe in this doctrine) by formerly continuous land, and the Cape Verde Islands from Africa; such colonists would be liable

to modification—the principle of inheritance still betraying their original birthplace.

The same principle which governs the general character of the inhabitants of oceanic islands, namely, the relation to the source whence colonists could have been most easily derived, together with their subsequent modification, is of the widest application throughout nature. We see this on every mountain summit, in every lake and marsh. For Alpine species, excepting in so far as the same species have become widely spread during the Glacial epoch, are related to those of the surrounding lowlands; thus we have in South America, Alpine hummingbirds, Alpine rodents, Alpine plants, &c., all strictly belonging to American forms; and it is obvious that a mountain, as it became slowly upheaved, would be colonised from the surrounding lowlands. So it is with the inhabitants of lakes and marshes, excepting in so far as great facility of transport has allowed the same forms to prevail throughout large portions of the world. We see this same principle in the character of most of the blind animals inhabiting the caves of America and of Europe. Other analogous facts could be given. It will, I believe, be found universally true that wherever in two regions, let them be ever so distant, many closely allied or representative species occur, there will likewise be found some identical species; and wherever many closely allied species occur, there will be found many forms which some naturalists rank as distinct species, and others as mere varieties; these doubtful forms showing us the steps in the progress of modification.

XIII. MUTUAL AFFINITIES OF ORGANIC BEINGS: MORPHOLOGY: EMBRYOLOGY

Classification

FROM the most remote period in the history of the world organic beings have been found to resemble each other in descending degrees, so that they can be classed in groups under groups. This classification is not arbitrary like the grouping of the stars in constellations. The existence of groups would have been of simpler significance if one group had been exclusively fitted to inhabit the land and another the water; one to feed on flesh, another on vegetable matter, and so on; but the case is widely different, for it is notorious how commonly members of even the same sub-group have different habits. In the second and fourth chapters, on Variation and on Natural Selection, I have attempted to show that within each country it is the widely ranging, the much diffused and common, that is the dominant species, belonging to the larger genera in each class, which vary most. The varieties, or incipient species, thus produced ultimately become converted into new and distinct species; and these, on the principle of inheritance, tend to produce other new and dominant species. Consequently the groups which are now large, and which generally include many dominant species, tend to go on increasing in size. I further

attempted to show that from the varying descendants of each species trying to occupy as many and as different places as possible in the economy of nature, they constantly tend to diverge in character. This latter conclusion is supported by observing the great diversity of forms which, in any small area, come into the closest competition, and by certain facts in naturalisation.

I attempted also to show that there is a steady tendency, in the forms which are increasing in number and diverging in character, to supplant and exterminate the preceding less divergent and less improved forms. I request the reader to turn to the diagram illustrating the action, as formerly explained, of these several principles; and he will see that the inevitable result is that the modified descendants proceeding from one progenitor become broken up into groups subordinate to groups.

Naturalists, as we have seen, try to arrange the species, genera, and families in each class on what is called the Natural System. But what is meant by this system? Some authors look at it merely as a scheme for arranging together those living objects which are most alike and for separating those which are most unlike; or as an artificial method of enunciating, as briefly as possible, general propositions—that is, by one sentence to give the characters common, for instance, to all mammals, by another those common to all carnivora, by another those common to the dog genus, and then, by adding a single sentence, a full description is given of each kind of dog. The ingenuity and utility of this system are indisputable. But many naturalists think that something more is meant by the Natural System; they believe that it reveals the plan of the Creator; but unless it be specified whether order in time or space, or both, or what else is meant by the plan of the Creator, it seems to me that nothing is thus added to our knowledge. Expressions such as that famous one by Linnæus, which we often meet with in a more or less concealed form, namely, that the characters do not make the genus but that the genus gives the characters, seem to imply that some deeper bond is included in our classifications than mere resemblance. I believe that this is the case, and that community of descent—the one known cause of close similarity in organic beings—is the bond which, though observed by various degrees of modification, is partially revealed to us by our classifications.

That the mere physiological importance of an organ does not determine its classificatory value is almost proved by the fact that in allied groups in which the same organ, as we have every reason to suppose, has nearly the same physiological value, its classificatory value is widely different. No naturalist can have worked long at any group without being struck with this fact; and it has been fully acknowledged in the writings of almost every author. To give an example amongst insects: in one great division of the Hymenoptera, the antennæ, as Westwood has remarked, are most constant in structure; in another division they differ much, and the differences are of quite subordinate value in classification; yet no one will say that the antennæ in these two divisions of the same order are of unequal physiological importance.

Numerous instances could be given of characters derived from parts which must be considered of very trifling physiological importance but which are universally admitted as highly serviceable in the definition of whole groups. For instance, whether or not there is an open passage from the nostrils to the mouth, the only character, according to Owen, which absolutely distinguishes fishes and reptiles—the inflection of the angle of the lower jaw in Marsupials—the manner in which the wings of insects are folded—mere colour in certain Algæ—mere pubescence on parts of the flower in grasses—the nature of the dermal covering, as hair or feathers in the Vertebrata. If the Ornithorhyncus had been covered with feathers instead of hair, this external and trifling character would have been considered by naturalists as an important aid in determining the degree of affinity of this strange creature to birds.

The importance, for classification, of trifling characters mainly depends on their being correlated with many other characters of more or less importance. The value indeed of an aggregate of characters is very evident in natural history. Hence, as has often been remarked, a species may depart from its allies in several characters, both of high physiological importance and of almost universal prevalence, and yet leave us in no doubt where it should be ranked. Hence, also, it has been found that a classification founded on any single character, however important that may be, has always failed; for no part of the organisation is invariably constant. The importance of an aggregate of characters, even when none are important, alone explains the aphorism enunciated by Linnæus, namely, that the characters do not give the genus, but the genus gives the characters; for this seems founded on the appreciation of many trifling points of resemblance, too slight to be defined.

We can see why characters derived from the embryo should be of equal importance with those derived from the adult, for a natural classification of course includes all ages. But it is by no means obvious, on the ordinary view, why the structure of the embryo should be more important for this purpose than that of the adult, which alone plays its full part in the economy of nature. Yet it has been strongly urged by those great naturalists, Milne Edwards and Agassiz, that embryological characters are the most important of all; and this doctrine has very generally been admitted as true. Thus the main divisions of flowering plants are founded on differences in the embryo—on the number and position of the cotyledons, and on the mode of development of the plumule and radicle. We shall immediately see why these characters possess so high a value in classification, namely, from the natural system being genealogical in its arrangement.

Our classifications are often plainly influenced by chains of affinities. Nothing can be easier than to define a number of characters common to all birds; but with crustaceans, any such definition has hitherto been found impossible. There are crustaceans at the opposite ends of the series, which have hardly a character in common; yet the species at both ends, from being plainly allied to others, and these to others, and so onwards,

can be recognised as unequivocally belonging to this and to no other class of the Articulata.

All the foregoing rules and aids and difficulties in classification may be explained, if I do not greatly deceive myself, on the view that the Natural System is founded on descent with modification;—that the characters which naturalists consider as showing true affinity between any two or more species are those which have been inherited from a common parent, all true classification being genealogical;—that community of descent is the hidden bond which naturalists have been unconsciously seeking, and not some unknown plan of creation, or the enunciation of general propositions, and the mere putting together and separating objects more or less alike.

But I must explain my meaning more fully. I believe that the *arrangement* of the groups within each class, in due subordination and relation to each other, must be strictly genealogical in order to be natural; but that the *amount* of difference in the several branches or groups, though allied in the same degree in blood to their common progenitor, may differ greatly, being due to the different degrees of modification which they have undergone; and this is expressed by the forms being ranked under different genera, families, sections, or orders. The reader will best understand what is meant if he will take the trouble to refer to the diagram in the fourth chapter. We will suppose the letters A to L to represent allied genera existing during the Silurian epoch, and descended from some still earlier form. In three of these genera (A, F, and I), a species has transmitted modified descendants to the present day, represented by the fifteen genera (a^{14} to z^{14}) on the uppermost horizontal line. Now all these modified descendants from a single species are related in blood or descent in the same degree; they may metaphorically be called cousins to the same millionth degree; yet they differ widely and in different degrees from each other. The forms descended from A, now broken up into two or three families, constitute a distinct order from those descended from I, also broken up into two families. Nor can the existing species, descended from A, be ranked in the same genus with the parent A; or those from I, with the parent I. But the existing genus F^{14} may be supposed to have been but slightly modified; and it will then rank with the parent genus F; just as some few still living organisms belong to Silurian genera. So that the comparative value of the differences between these organic beings, which are all related to each other in the same degree in blood, has come to be widely different. Nevertheless their genealogical *arrangement* remains strictly true, not only at the present time, but at each successive period of descent. All the modified descendants from A will have inherited something in common from their common parent, as will all the descendants from I; so will it be with each subordinate branch of descendants, at each successive stage. If, however, we suppose any descendant of A, or of I, to have become so much modified as to have lost all traces of its parentage, in this case, its place in the natural system will be lost, as seems to have occurred with some few existing organisms. All

the descendants of the genus F, along its whole line of descent, are supposed to have been but little modified, and they form a single genus. But this genus, though much isolated, will still occupy its proper intermediate position.

As descent has universally been used in classing together the individuals of the same species, though the males and females and larvæ are sometimes extremely different; and as it has been used in classing varieties which have undergone a certain, and sometimes a considerable amount of modification, may not this same element of descent have been unconsciously used in grouping species under genera, and genera under higher groups, all under the so-called natural system? I believe it has been unconsciously used; and thus only can I understand the several rules and guides which have been followed by our best systematists. As we have no written pedigrees, we are forced to trace community of descent by resemblances of any kind. Therefore we chose those characters which are the least likely to have been modified, in relation to the conditions of life to which each species has been recently exposed. Rudimentary structures on this view are as good as, or even better than, other parts of the organisation. We care not how trifling a character may be—let it be the mere inflection of the angle of the jaw, the manner in which an insect's wing is folded, whether the skin be covered by hair or feathers—if it prevail throughout many and different species, especially those having very different habits of life, it assumes high value; for we can account for its presence in so many forms with such different habits only by inheritance from a common parent. We may err in this respect in regard to single points of structure, but when several characters, let them be ever so trifling, concur throughout a large group of beings having different habits, we may feel almost sure, on the theory of descent, that these characters have been inherited from a common ancestor; and we know that such aggregated characters have especial value in classification.

On the principle of the multiplication and gradual divergence in character of the species descended from a common progenitor, together with their retention by inheritance of some characters in common, we can understand the excessively complex and radiating affinities by which all the members of the same family or higher group are connected together. For the common progenitor of a whole family, now broken up by extinction into distinct groups and sub-groups, will have transmitted some of its characters, modified in various ways and degrees, to all the species; and they will consequently be related to each other by circuitous lines of affinity of various lengths (as may be seen in the diagram so often referred to), mounting up through many predecessors. As it is difficult to show the blood relationship between the numerous kindred of any ancient and noble family even by the aid of a genealogical tree, and almost impossible to do so without this aid, we can understand the extraordinary difficulty which naturalists have experienced in describing, without the aid of a diagram, the various affinities which they perceive between the many living and extinct members of the same great natural class.

Extinction has played an important part in defining and widening the intervals between the several groups in each class. We may thus account for the distinctness of whole classes from each other—for instance, of birds from all other vertebrate animals—by the belief that many ancient forms of life have been utterly lost, through which the early progenitors of birds were formerly connected with the early progenitors of the other and at that time less differentiated vertebrate classes. There has been much less extinction of the forms of life which once connected fishes with batrachians. There has been still less within some whole classes, for instance the Crustacea, for here the most wonderfully diverse forms are still linked together by a long and only partially broken chain of affinities. Extinction has only defined the groups: it has by no means made them; for if every form which has ever lived on this earth were suddenly to reappear, though it would be quite impossible to give definitions by which each group could be distinguished, still a natural classification, or at least a natural arrangement, would be possible.

Morphology

We have seen that the members of the same class, independently of their habits of life, resemble each other in the general plan of their organisation. This resemblance is often expressed by the term "unity of type," or by saying that the several parts and organs in the different species of the class are homologous. The whole subject is included under the general term of Morphology. This is one of the most interesting departments of natural history, and may almost be said to be its very soul. What can be more curious than that the hand of a man, formed for grasping, that of a mole for digging, the leg of the horse, the paddle of the porpoise, and the wing of the bat should all be constructed on the same pattern, and should include similar bones, in the same relative positions? How curious it is, to give a subordinate though striking instance, that the hind feet of the kangaroo, which are so well fitted for bounding over the open plains—those of the climbing, leaf-eating koala, equally well fitted for grasping the branches of trees—those of the ground-dwelling, insect or root-eating, bandicoots—and those of some other Australian marsupials—should all be constructed on the same extraordinary type, namely with the bones of the second and third digits extremely slender and enveloped within the same skin, so that they appear like a single toe furnished with two claws. Notwithstanding this similarity of pattern, it is obvious that the hind feet of these several animals are used for as widely different purposes as it is possible to conceive. The case is rendered all the more striking by the American opossums, which follow nearly the same habits of life as some of their Australian relatives, having feet constructed on the ordinary plan.

The explanation is to a large extent simple on the theory of the selection of successive slight modifications—each modification being profitable in some way to the modified form, but often affecting by correlation

other parts of the organisation. In changes of this nature, there will be little or no tendency to alter the original pattern or to transpose the parts. The bones of a limb might be shortened and flattened to any extent, becoming at the same time enveloped in thick membrane, so as to serve as a fin; or a webbed hand might have all its bones, or certain bones, lengthened to any extent, with the membrane connecting them increased, so as to serve as a wing; yet all these modifications would not tend to alter the framework of the bones or the relative connection of the parts. If we suppose that an early progenitor—the archetype, as it may be called—of all mammals, birds, and reptiles had its limbs constructed on the existing general pattern, for whatever purpose they served, we can at once perceive the plain signification of the homologous construction of the limbs throughout the class.

There is another and equally curious branch of our subject; namely, serial homologies, or the comparison of the different parts or organs in the same individual, and not of the same parts or organs in different members of the same class. Most physiologists believe that the bones of the skull are homologous—that is, correspond in number and in relative connection—with the elemental parts of a certain number of vertebræ. The anterior and posterior limbs in all the higher vertebrate classes are plainly homologous. So it is with the wonderfully complex jaws and legs of crustaceans.

How inexplicable are the cases of serial homologies on the ordinary view of creation! Why should the brain be enclosed in a box composed of such numerous and such extraordinarily shaped pieces of bone, apparently representing vertebræ? As Owen has remarked, the benefit derived from the yielding of the separate pieces in the act of parturition by mammals will by no means explain the same construction in the skulls of birds and reptiles. Why should similar bones have been created to form the wing and the leg of a bat, used as they are for such totally different purposes, namely flying and walking?

On the theory of natural selection, we can, to a certain extent, answer these questions. We need not here consider how the bodies of some animals first became divided into a series of segments, or how they became divided into right and left sides, with corresponding organs, for such questions are almost beyond investigation. It is, however, probable that some serial structures are the result of cells multiplying by division, entailing the multiplication of the parts developed from such cells. It must suffice for our purpose to bear in mind that an indefinite repetition of the same part or organ is the common characteristic, as Owen has remarked, of all low or little specialised forms; therefore the unknown progenitor of the Vertebrata probably possessed many vertebræ; the unknown progenitor of the Articulata, many segments; and the unknown progenitor of flowering plants, many leaves arranged in one or more spires. We have also formerly seen that parts many times repeated are eminently liable to vary, not only in number, but in form. Consequently such parts, being already present in considerable numbers, and being highly variable, would

naturally afford the materials for adaptation to the most different purposes; yet they would generally retain, through the force of inheritance, plain traces of their original or fundamental resemblance. They would retain this resemblance all the more, as the variations which afforded the basis for their subsequent modification through natural selection would tend from the first to be similar; the parts being at an early stage of growth alike, and being subjected to nearly the same conditions. Such parts, whether more or less modified, unless their common origin became wholly obscured, would be serially homologous.

Development and Embryology

This is one of the most important subjects in the whole round of history. The metamorphoses of insects, with which everyone is familiar, are generally effected abruptly by a few stages; but the transformations are in reality numerous and gradual, though concealed. A certain ephemerous insect (Chlöeon) during its development moults, as shown by Sir J. Lubbock, above twenty times, and each time undergoes a certain amount of change; and in this case we see the act of metamorphosis performed in a primary and gradual manner. Many insects, and especially certain crustaceans, show us what wonderful changes of structure can be effected during development. Such changes, however, reach their acme in the so-called alternate generations of some of the lower animals. It is, for instance, an astonishing fact that a delicate branching coralline, studded with polypi and attached to a submarine rock, should produce, first by budding and then by transverse division, a host of huge floating jelly-fishes; and that these should produce eggs, from which are hatched swimming animalcules, which attach themselves to rocks and become developed into branching corallines; and so on in an endless cycle.

It has already been stated that various parts in the same individual which are exactly alike during an early embryonic period become widely different and serve for widely different purposes in the adult state. So again it has been shown that generally the embryos of the most distinct species belonging to the same class are closely similar, but become, when fully developed, widely dissimilar. The larvæ of most crustaceans, at corresponding stages of development, closely resemble each other, however different the adult may become; and so it is with very many other animals. A trace of the law of embryonic resemblance occasionally lasts till a rather late age: thus birds of the same genus, and of allied genera, often resemble each other in their immature plumage; as we see in the spotted feathers in the young of the thrush group. In the cat tribe, most of the species when adult are striped or spotted in lines; and stripes or spots can be plainly distinguished in the whelp of the lion and the puma.

It is commonly assumed, perhaps from monstrosities affecting the embryo at a very early period, that slight variations or individual differences necessarily appear at an equally early period. We have little evidence

on this head, but what we have certainly points the other way; for it is notorious that breeders of cattle, horses, and various fancy animals cannot positively tell, until some time after birth, what will be the merits or demerits of their young animals. We see this plainly in our own children; we cannot tell whether a child will be tall or short, or what its precise features will be. The question is not at what period of life each variation may have been caused, but at what period the effects are displayed. The cause may have acted, and I believe often has acted, on one or both parents before the act of generation. It deserves notice that it is of no importance to a very young animal, as long as it remains in its mother's womb or in the egg, or as long as it is nourished and protected by its parent, whether most of its characters are acquired a little earlier or later in life. It would not signify, for instance, to a bird which obtained its food by having a much-curved beak whether or not whilst young it possessed a beak of this shape, as long as it was fed by its parents.

I have stated in the first chapter that at whatever age a variation first appears in the parent, it tends to reappear at a corresponding age in the offspring. Certain variations can only appear at corresponding ages; for instance, peculiarities in the caterpillar, cocoon, or imago states of the silk moth; or, again, in the full-grown horns of cattle. But variations, which, for all that we can see might have first appeared either earlier or later in life, likewise tend to reappear at a corresponding age in the offspring and parent. I am far from meaning that this is invariably the case, and I could give several exceptional cases of variations (taking the word in the largest sense) which have supervened at an earlier age in the child than in the parent.

These two principles, namely, that slight variations generally appear at a not very early period of life and are inherited at a corresponding not early period, explain, as I believe, all the above-specified leading facts in embryology. But first let us look to an analogous case in our domestic varieties. Some authors who have written on Dogs maintain that the greyhound and bulldog, though so different, are really closely allied varieties, descended from the same wild stock; hence I was curious to see how far their puppies differed from each other: I was told by breeders that they differed just as much as their parents, and this, judging by the eye, seemed almost to be the case; but on actually measuring the old dogs and their six-days-old puppies, I found that the puppies had not acquired nearly their full amount of proportional difference.

If, on the other hand, it profited the young of an animal to follow habits of life slightly different from those of the parent form, and consequently to be constructed on a slightly different plan, or if it profited a larva already different from its parent to change still further, then, on the principle of inheritance at corresponding ages, the young or the larvæ might be rendered by natural selection more and more different from their parents to any conceivable extent. Differences in the larva might, also, become correlated with successive stages of its development; so that the larva in the first stage might come to differ greatly from the larva in

the second stage, as is the case with many animals. The adult might also become fitted for sites or habits, in which organs of locomotion or of the senses, &c., would be useless; and in this case the metamorphosis would be retrograde.

From the remarks just made we can see how by changes of structure in the young, in conformity with changed habits of life, together with inheritance at corresponding ages, animals might come to pass through stages of development, perfectly distinct from the primordial condition of their adult progenitors. Most of our best authorities are now convinced that the various larval and pupal stages of insects have thus been acquired through adaptation, and not through inheritance from some ancient form.

Now let us apply these two principles to species in a state of nature. Let us take a group of birds, descended from some ancient form and modified through natural selection for different habits. Then, from the many slight successive variations having supervened in the several species at a not early age, and having been inherited at a corresponding age, the young will have been but little modified, and they will still resemble each other much more closely than do the adults. We may extend this view to widely distinct structures and to whole classes. The forelimbs, for instance, which once served as legs to a remote progenitor, may have become, through a long course of modification, adapted in one descendant to act as hands, in another as paddles, in another as wings; but on the above two principles the forelimbs will not have been much modified in the embryos of these several forms; although in each form the forelimb will differ greatly in the adult state. Whatever influence long-continued use or disuse may have had in modifying the limbs or other parts of any species, this will chiefly or solely have affected it when nearly mature, when it was compelled to use its full powers to gain its own living; and the effects thus produced will have been transmitted to the offspring at a corresponding nearly mature age. Thus the young will not be modified, or will be modified only in a slight degree, through the effects of the increased use or disuse of parts.

As all the organic beings, extinct and recent, which have ever lived can be arranged within a few great classes; and as all within each class have, according to our theory, been connected together by fine gradations, the best, and, if our collections were nearly perfect, the only possible arrangement would be genealogical; descent being the hidden bond of connection which naturalists have been seeking under the term of the Natural System. On this view we can understand how it is that in the eyes of most naturalists the structure of the embryo is even more important for classification than that of the adult. In two or more groups of animals, however much they may differ from each other in structure and habits in their adult condition, if they pass through closely similar embryonic stages, we may feel assured that they all are descended from one parent form and are therefore closely related. Thus community in embryonic structure reveals community of descent; but dissimilarity in embryonic development does not prove discommunity of descent, for in one of two

groups the developmental stages may have been suppressed, or may have been so greatly modified through adaptation to new habits of life as to be no longer recognisable. Even in groups, in which the adults have been modified to an extreme degree, community of origin is often revealed by the structure of the larvæ; cirripedes, though externally so like shellfish, are at once known by their larvæ to belong to the great class of crustaceans. As the embryo often shows us more or less plainly the structure of the less modified and ancient progenitor of the group, we can see why ancient and extinct forms so often resemble in their adult state the embryos of existing species of the same class. Agassiz believes this to be a universal law of nature; and we may hope hereafter to see the law proved true. It can, however, be proved true only in those cases in which the ancient state of the progenitor of the group has not been wholly obliterated, either by successive variations having supervened at a very early period of growth or by such variations having been inherited at an earlier age than that at which they first appeared. It should also be borne in mind that the law may be true, but yet, owing to the geological record not extending far enough back in time, may remain for a long period, or forever, incapable of demonstration. The law will not strictly hold good in those cases in which an ancient form became adapted in its larvæ state to some special line of life and transmitted the same larval state to a whole group of descendants; for such larvæ will not resemble any still more ancient form in its adult state.

Thus, as it seems to me, the leading facts in embryology, which are second to none in importance, are explained on the principle of variations in the many descendants from some one ancient progenitor, having appeared at a not very early period of life, and having been inherited at a corresponding period. Embryology rises greatly in interest when we look at the embryo as a picture, more or less obscured, of the progenitor, either in its adult or larval state, of all the members of the same great class.

XIV. CONCLUSION

THAT many and serious objections may be advanced against the theory of descent with modification through variation and natural selection, I do not deny. I have endeavoured to give to them their full force. Nothing at first can appear more difficult to believe than that the more complex organs and instincts have been perfected, not by means superior to, though analogous with, human reason, but by the accumulation of innumerable slight variations, each good for the individual possessor. Nevertheless, this difficulty, though appearing to our imagination insuperably great, cannot be considered real if we admit the following propositions, namely, that all parts of the organisation and instincts offer, at least, individual differences—that there is a struggle for existence leading to the preservation of profitable deviations of structure or instinct—and, lastly, that gradations in the state of perfection of each organ may have existed, each

good of its kind. The truth of these propositions cannot, I think, be disputed.

It may be asked how far I extend the doctrine of the modification of species. The question is difficult to answer, because the more distinct the forms are which we consider, by so much the arguments in favour of community of descent become fewer in number and less in force. But some arguments of the greatest weight extend very far. All the members of whole classes are connected together by a chain of affinities, and all can be classed on the same principle, in groups subordinate to groups. Fossil remains sometimes tend to fill up very wide intervals between existing orders.

Organs in a rudimentary condition plainly show that an early progenitor had the organ in a fully developed condition; and this in some cases implies an enormous amount of modification in the descendants. Throughout whole classes various structures are formed on the same pattern, and at a very early age the embryos closely resemble each other. Therefore I cannot doubt that the theory of descent with modification embraces all the members of the same great class or kingdom. I believe that animals are descended from at most only four or five progenitors, and plants from an equal or lesser number.

Analogy would lead me one step farther, namely, to the belief that all animals and plants are descended from some one prototype. But analogy may be a deceitful guide. Nevertheless all living things have much in common, in their chemical composition, their cellular structure, their laws of growth, and their liability to injurious influences. We see this even in so trifling a fact as that the same poison often similarly affects plants and animals; or that the poison secreted by the gallfly produces monstrous growths on the wild rose or oak tree. With all organic beings, excepting perhaps some of the very lowest, sexual production seems to be essentially similar. With all, as far as is at present known, the germinal vesicle is the same; so that all organisms start from a common origin. If we look even to the two main divisions—namely, to the animal and vegetable kingdoms—certain low forms are so far intermediate in character that naturalists have disputed to which kingdom they should be referred. As Professor Asa Gray has remarked, "the spores and other reproductive bodies of many of the lower algæ may claim to have first a characteristically animal and then an unequivocally vegetable existence." Therefore, on the principle of natural selection with divergence of character, it does not seem incredible that, from such low and intermediate form, both animals and plants may have been developed; and, if we admit this, we must likewise admit that all the organic beings which have ever lived on this earth may be descended from some one primordial form. But this inference is chiefly grounded on analogy and it is immaterial whether or not it be accepted.

When the views advanced by me in this volume, and by Mr. Wallace, or when analogous views on the origin of species are generally admitted, we can dimly foresee that there will be a considerable revolution in natural

history. The other and more general departments of natural history will rise greatly in interest. The terms used by naturalists, of affinity, relationship, community of type, paternity, morphology, adaptive characters, rudimentary and aborted organs, &c., will cease to be metaphorical, and will have a plain signification. When we no longer look at an organic being as a savage looks at a ship, as something wholly beyond his comprehension; when we regard every production of nature as one which has had a long history; when we contemplate every complex structure and instinct as the summing up of many contrivances, each useful to the possessor, in the same way as any great mechanical invention is the summing up of the labour, the experience, the reason, and even the blunders of numerous workmen; when we thus view each organic being, how far more interesting—I speak from experience—does the study of natural history become!

A grand and almost untrodden field of inquiry will be opened, on the causes and laws of variation, on correlation, on the effects of use and disuse, on the direct action of external conditions, and so forth. The study of domestic productions will rise immensely in value. A new variety raised by man will be a more important and interesting subject for study than one more species added to the infinitude of already recorded species. Our classifications will come to be, as far as they can be so made, genealogies; and will then truly give what may be called the plan of creation. The rules for classifying will no doubt become simpler when we have a definite object in view. We possess no pedigrees or armorial bearings; and we have to discover and trace the many diverging lines of descent in our natural genealogies, by characters of any kind which have long been inherited. Rudimentary organs will speak infallibly with respect to the nature of long-lost structures. Species and groups of species which are called aberrant, and which may fancifully be called living fossils, will aid us in forming a picture of the ancient forms of life. Embryology will often reveal to us the structure, in some degree obscured, of the prototype of each great class.

Authors of the highest eminence seem to be fully satisfied with the view that each species has been independently created. To my mind it accords better with what we know of the laws impressed on matter by the Creator that the production and extinction of the past and present inhabitants of the world should have been due to secondary causes, like those determining the birth and death of the individual. When I view all beings not as special creations, but as the lineal descendants of some few beings which lived long before the first bed of the Cambrian system was deposited, they seem to me to become ennobled. Judging from the past, we may safely infer that not one living species will transmit its unaltered likeness to a distant futurity. And of the species now living very few will transmit progeny of any kind to a far-distant futurity; for the manner in which all organic beings are grouped shows that the greater number of species in each genus, and all the species in many genera, have left no descendants, but have become utterly extinct. We can so far take a prophetic glance into futurity as to foretell that it will be the common and

widely spread species, belonging to the larger and dominant groups within each class, which will ultimately prevail and procreate new and dominant species. As all the living forms of life are the lineal descendants of those which lived long before the Cambrian epoch, we may feel certain that the ordinary succession by generation has never once been broken, and that no cataclysm has desolated the whole world. Hence we may look with some confidence to a secure future of great length. And as natural selection works solely by and for the good of each being, all corporeal and mental endowments will tend to progress towards perfection.

It is interesting to contemplate a tangled bank, clothed with many plants of many kinds, with birds singing on the bushes, with various insects flitting about, and with worms crawling through the damp earth, and to reflect that these elaborately constructed forms, so different from each other, and dependent upon each other in so complex a manner, have all been produced by laws acting around us. These laws, taken in the largest sense, being Growth with Reproduction; Inheritance which is almost implied by reproduction; Variability from the indirect and direct action of the conditions of life, and from use and disuse: a Ratio of Increase so high as to lead to a Struggle for Life, and as a consequence to Natural Selection, entailing Divergence of Character and the Extinction of less-improved forms. Thus, from the war of nature, from famine and death, the most exalted object which we are capable of conceiving, namely, the production of the higher animals, directly follows. There is grandeur in this view of life, with its several powers, having been originally breathed by the Creator into a few forms or into one; and that, whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being, evolved.

EXPERIMENTAL RESEARCHES IN ELECTRICITY

by

MICHAEL FARADAY

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MICHAEL FARADAY

1791–1867

THE FARADAYS cherished a tradition that a progenitor had come from Ireland into northern England. There, in Westmorland, the family first appears in eighteenth-century records as members of a Sandemanian (or Glassite) congregation. James Faraday, born in 1761, became a blacksmith, married Margaret Hastwell, and removed to London. His four children were born in London or in Surrey, near by. Michael, the third child, was born in Newington, Surrey, in 1791. Because James never earned much, and was besides afflicted in health, the children grew up in poverty. Michael later reported that his formal education was confined to instruction in the three Rs at a common day school, and that he spent his free hours at home or in the street. When he was twelve he entered the service of a bookbinder and bookseller as errand boy, having practically the duties of a newspaper boy. The following year he was apprenticed to the same bookbinder for a seven-year period.

Faraday's true education began during these years of apprenticeship. He read the books he was binding, he attended the evening lectures of Mr. Tatam, founder of a mutual improvement group called the City Philosophical Society, and he cultivated the acquaintance of several young men who shared his ardency for education and self-improvement. Of his reading, Faraday wrote that he "delighted in Marcet's *Conversations in Chemistry,* and the electrical treatises in the *Encyclopaedia Britannica.*" In his lodgings with his master, he performed such simple experiments in chemistry as he could finance for a few pennies, and he actually built an electrical machine and other bits of electrical apparatus.

About the time Faraday's apprenticeship was ending, in 1812, a customer of his master's took him to hear Sir Humphry Davy lecture at the Royal Institution. The young

man's interest was so fired that he attended more of Davy's lectures, took notes on them, elaborated and illustrated his notes with drawings, and resolved to become a scientist rather than a bookbinder. He had the audacity to write to Sir Humphry, asking for an opportunity to work in science, and he enclosed his illustrated lecture notes. Sir Humphry was impressed. He talked to the young man, liked him, advised him not to devote his life to science—which he described as a hard mistress—laughed at his notion that men of science had the highest and purest moral motives, and offered him an assistantship at the Royal Institution. An assistant was granted a salary of twenty-five shillings a week and two rooms at the top of the house. Faraday accepted the offer.

Davy used Faraday's services in his experiments with the explosive nitrogen trichloride, and thought so well of him that when, in the fall of 1813, he decided to go to the Continent for an extended tour, he took him along as his amanuensis. Unfortunately, Davy's valet withdrew at the last moment, and his duties fell upon Faraday. He squirmed; but he persisted in completing the trip with Sir Humphry and Lady Davy, for he was aware that association with the great man was in itself an education. So for two years he traveled through France and Switzerland and the Tyrol and Italy, studying French and Italian diligently, assisting Sir Humphry in the performance of experiments and demonstrations—one of the most fascinating was the burning, at Florence, of a diamond in an atmosphere of oxygen, using the great lenses belonging to the Duke of Florence as a burning glass—making the acquaintance of such scientists as Volta and the elder de la Rive, and writing voluminous letters home to England promising that once back there he would never leave again.

In 1815, Sir Humphry and his party returned to England. Very soon Faraday was back at the Royal Institution as a laboratory assistant and superintendent of apparatus. He had now thirty shillings a week and decent living quarters. At once he called in his old friends of the apprentice days to continue with him their activities for mutual educational improvement. Now a member of the City Philosophical Society, he lectured before it for the first time in 1816, his subject being "The General Properties of Matter." Thereafter he studied elocution with a teacher and devoted some part of his attention to means of making himself a good lecturer. As one of his duties at the Institution, he attended all lectures given there. How much he profited by observing the methods of successful speakers appears from his own success when he first lectured at the Institution in 1827. His series begun then continued for more than thirty years. Europe has never had in

science a more practiced, brilliant, and successful popular instructor.

In 1816, Faraday made his first positive contribution to scientific literature, an analysis of native caustic lime from Tuscany. He printed the paper in the *Quarterly Journal of Science;* in the same *Journal,* during the next four years, he printed thirty-seven articles and notes. These were on various subjects in physics and chemistry, for he had not yet settled upon electricity as his special interest. In 1823 he succeeded in liquefying chlorine. The experiment had grown out of a suggestion of Davy's; and Davy claimed that credit for the accomplishment belonged to him. Only Faraday's modesty and disinterestedness prevented a break between the two men. In the following year, when Faraday was nominated to a fellowship in the Royal Society, Davy displayed ill will often interpreted as jealousy. Despite his opposition, Faraday was elected, and the friction between the two lessened quickly. In 1825 it was Davy who nominated Faraday to become the director of the laboratory at the Royal Institution.

While investigating the condensed oil gas manufactured by the Portable Gas Company, Faraday discovered benzol (which he called bicarburet of hydrogen). Some of his biographers have made the rather extravagant claim that he is therefore responsible for the whole enormous aniline trade industry. About the same time, in 1829, he became a lecturer at the Royal Military Academy at Woolwich and a member of the Scientific Advising Committee of the Admiralty. A few years later he became Scientific Adviser to Trinity House. Almost until his death he retained these connections with the government, generally receiving no stipend for the advice and decisions he gave on the ventilation of lighthouses, the purchase and manufacture of optical equipment, the selection of paints, cottons, oils, lightning conductors for lighthouses, and so on. He thought that a good subject owed such services to his government.

Though he had begun some experiments on magnetism and electricity as early as 1823, not until some years later did Faraday devote himself to the great experiments in electricity for which he is principally famous. In 1831 he discovered electromagnetic induction. His results he gave to the Royal Society in his First Series of *Experimental Researches in Electricity.* In the years following he contributed further series of papers regularly. From these the following selections are taken.

The publication of the *Experimental Researches in Electricity* established Faraday's reputation with the non-scientific world. Immediately commerce and industry began to bid for his services. In the next year, by his advisory work, he added

a full thousand pounds to his two-hundred-pound stipend from the Institution. The following year he earned more. Then he made up his mind that he could serve only one master, and that he preferred science to wealth. In subsequent years he accepted employment apart from the pure research and lecturing of the Institution at a sharply declining rate; after 1845 he never accepted a penny for any industrial work.

Faraday had married in 1821 the daughter of an elder in the Sandemanian Congregation of which he was a member. With her he lived a life compounded of the sweetest sympathy and understanding. Both were extremely devout, for Faraday kept his religious convictions—the Sandemanians held to doctrines which would now be labeled fundamentalist—and his scientific views strictly apart. In 1840 he was elected an elder in his church, and thus had pressed upon him the duty of preaching a sermon on alternate Sundays. Possibly it was this addition to his already great intellectual load which caused him to suffer a partial breakdown in 1841. He suffered particularly from loss of memory; he had to take a long holiday, and for three years he abandoned his studies.

The long vacation obviously did not impair his powers. In 1845 he discovered the influence of a magnetic field of force on polarized light, and in the same year he established the distinction between magnetic and diamagnetic substances. The two great accomplishments won for him, in 1846, from the Royal Society, both the Royal and the Rumford medals. Such honors were by this time no novelties to him. Perhaps, indeed, no other scientist has ever been equally recognized, feted, and decorated in his own lifetime. He received no fewer than ninety-five honorary titles and marks of distinction from the learned societies of Europe and America. He deserved them. For among the incredibly numerous discoveries credited to him, four must be characterized as massive: magneto-electric induction, the chemical phenomena of the electric current, the magnetization of light, and diamagnetism. And only compared with these are his studies in the liquefaction of gases, in frictional electricity, in regelation, of small importance.

Twice Faraday did refuse honors. In 1835 he refused a government pension—which he was subsequently persuaded to accept. And twice he declined to become president of the Royal Society. He did not refuse the house on Hampton Court Green which Queen Victoria, through the good offices of the Prince Consort, offered him in 1858. There he spent his declining years. In 1865 he made his last report to Trinity House and relinquished his duties at the Royal Institution. In 1867 he died.

Faraday's skill as an experimenter and his success as a

lecturer depended in no small degree on his remarkable sense of order, his pertinacity, and his control over a kind of Celtic impulsiveness. His biographer Tyndall, who was his successor at the Royal Institution and his great personal friend, remarks that the man was never swallowed up in the scientist. He speaks eloquently of Faraday's long friendships with Davy, Biot, the two de la Rives, Arago, Humboldt, and a host of students and assistants. To sum up the man, Tyndall quotes from St. Paul: "blameless, vigilant, sober, of good behaviour, apt to teach, not given to filthy lucre."

EXPERIMENTAL RESEARCHES IN ELECTRICITY

I. IDENTITY OF ELECTRICITIES DERIVED FROM DIFFERENT SOURCES

THE PROGRESS of the electrical researches which I have had the honour to present to the Royal Society brought me to a point at which it was essential for the further prosecution of my inquiries that no doubt should remain of the identity or distinction of electricities excited by different means. I have satisfied myself that they are identical, and I hope the experiments which I have to offer, and the proofs flowing from them, will be found worthy the attention of the Royal Society.

The various phenomena exhibited by electricity may, for the purposes of comparison, be arranged under two heads; namely, those connected with electricity of tension, and those belonging to electricity in motion. This distinction is taken at present not as philosophical, but merely as convenient. The effect of electricity of tension, at rest, is either attraction or repulsion at sensible distances. The effects of electricity in motion or electrical currents may be considered as 1st, Evolution of heat; 2nd, Magnetism; 3rd, Chemical decomposition; 4th, Physiological phenomena; 5th, Spark. It will be my object to compare electricities from different sources, and especially common and voltaic electricities, by their power of producing these effects.

1. Voltaic Electricity

Tension.—When a voltaic battery of 100 pairs of plates has its extremities examined by the ordinary electrometer, it is well known that they are found positive and negative, the gold leaves at the same extremity repelling each other, the gold leaves at different extremities attracting each other, even when half an inch or more of air intervenes.

That ordinary electricity is discharged by points with facility through air, that it is readily transmitted through highly rarefied air, and also through heated air, as for instance a flame, is due to its high tension. I sought, therefore, for similar effects in the discharge of voltaic electricity, using as a test of the passage of the electricity either the galvanometer or chemical action produced by the arrangement hereafter to be described.

The voltaic battery I had at my disposal consisted of 140 pairs of

plates four inches square, with double coppers. It was insulated throughout, and diverged a gold-leaf electrometer about one third of an inch. On endeavouring to discharge this battery by delicate points very nicely arranged and approximated, either in the air or in an exhausted receiver, I could obtain no indications of a current, either by magnetic or chemical action. In this, however, was found no point of discordance between voltaic and common electricity; for when a Leyden battery was charged so as to deflect the gold-leaf electrometer to the same degree, the points were found equally unable to discharge it with such effect as to produce either magnetic or chemical action. This was not because common electricity could not produce both these effects, but because when of such low intensity the quantity required to make the effects visible (being enormously great) could not be transmitted in any reasonable time. In conjunction with the other proofs of identity hereafter to be given, these effects of points also prove identity instead of difference between voltaic and common electricity.

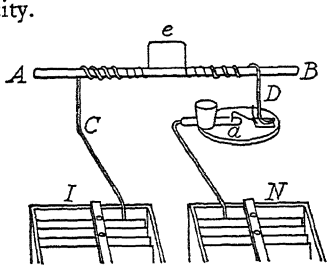

FIG. 1.

As heated air discharges common electricity with far greater facility than points, I hoped that voltaic electricity might in this way also be discharged. An apparatus was therefore constructed (Fig. 1), in which A B is an insulated glass rod upon which two copper wires, C, D, are fixed firmly; to these wires are soldered two pieces of fine platina wire, the ends of which are brought very close to each other at *e*, but without touching; the copper wire C was connected with the positive pole of a voltaic battery, and the wire D with a decomposing apparatus, from which the communication was completed to the negative pole of the battery. In these experiments only two troughs, or twenty pairs of plates, were used.

Whilst in the state described, no decomposition took place at the point *a*, but when the side of a spirit-lamp flame was applied to the two platina extremities at *e*, so as to make them bright red-hot, decomposition occurred; iodine soon appeared at the point *a*, and the transference of electricity through the heated air was established. On raising the temperature of the points *e* by a blowpipe, the discharge was rendered still more free, and decomposition took place instantly. On removing the source of heat, the current immediately ceased. On putting the ends of the wires very close by the side of and parallel to each other, but not touching, the

effects were perhaps more readily obtained than before. On using a larger voltaic battery, they were also more freely obtained.

These effects, not hitherto known or expected under this form, are only cases of the discharge which takes place through air between the charcoal terminations of the poles of a powerful battery, when they are gradually separated after contact. Then the passage is through heated air exactly as with common electricity, and Sir H. Davy has recorded that with the original battery of the Royal Institution this discharge passed through a space of at least four inches. In the exhausted receiver the electricity would *strike* through nearly half an inch of space, and the combined effect of rarefaction and heat was such upon the inclosed air as to enable it to conduct the electricity through a space of six or seven inches.

The instantaneous charge of a Leyden battery by the poles of a voltaic apparatus is another proof of the tension, and also the quantity, of electricity evolved by the latter. Sir H. Davy says, "When the two conductors from the ends of the combination were connected with a Leyden battery, one with the internal, the other with the external coating, the battery instantly became charged; and on removing the wires and making the proper connections, either a shock or a *spark* could be perceived: and the least possible time of contact was sufficient to renew the charge to its full intensity."

In motion: i. *Evolution of heat.*—The evolution of heat in wires and fluids by the voltaic current is matter of general notoriety.

ii. *Magnetism.*—No fact is better known to philosophers than the power of the voltaic current to deflect the magnetic needle, and to make magnets according to *certain laws;* and no effect can be more distinctive of an electrical current.

iii. *Chemical decomposition.*—The chemical powers of the voltaic current, and their subjection to *certain laws,* are also perfectly well known.

iv. *Physiological effects.*—The power of the voltaic current, when strong, to shock and convulse the whole animal system, and when weak to affect the tongue and the eyes, is very characteristic.

v. *Spark.*—The brilliant star of light produced by the discharge of a voltaic battery is known to all as the most beautiful light that man can produce by art.

That these effects may be almost infinitely varied, some being exalted whilst others are diminished, is universally acknowledged; and yet without any doubt of the identity of character of the voltaic currents thus made to differ in their effect. The beautiful explication of these variations afforded by Cavendish's theory of quantity and intensity requires no support at present, as it is not supposed to be doubted.

In consequence of the comparisons that will hereafter arise between wires carrying voltaic and ordinary electricities, and also because of certain views of the condition of a wire or any other conducting substance connecting the poles of a voltaic apparatus, it will be necessary to give some definite expression of what is called the voltaic current, in contradistinction to any supposed peculiar state of arrangement, not progressive,

which the wire or the electricity within it may be supposed to assume. If two voltaic troughs P N, P′ N′, Fig. 2, be symmetrically arranged and insulated, and the ends N P′ connected by a wire, over which a magnetic needle is suspended, the wire will exert no effect over the needle; but immediately that the ends P N′ are connected by another wire, the needle will be deflected, and will remain so as long as the circuit is complete. Now if the troughs merely act by causing a peculiar arrangement in the wire either of its particles or its electricity, that arrangement constituting its electrical and magnetic state, then the wire N P′ should be in a similar state of arrangement *before* P and N′ were connected to what it is afterwards, and should have deflected the needle, although less powerfully, perhaps to one half the extent which would result when the communication is complete throughout. But if the magnetic effects depend upon a current, then it is evident why they could not be produced in *any* degree

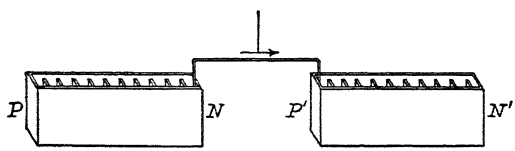

FIG. 2.

before the circuit was complete; because prior to that no current could exist.

By *current,* I mean anything progressive, whether it be a fluid of electricity, or two fluids moving in opposite directions, or merely vibrations, or, speaking still more generally, progressive forces. By *arrangement,* I understand a local adjustment of particles, or fluids, or forces, not progressive. Many other reasons might be urged in support of the view of a *current* rather than an *arrangement,* but I am anxious to avoid stating unnecessarily what will occur to others at the moment.

2. *Ordinary Electricity*

By ordinary electricity I understand that which can be obtained from the common machine, or from the atmosphere, or by pressure, or cleavage of crystals, or by a multitude of other operations; its distinctive character being that of great intensity, and the exertion of attractive and repulsive powers, not merely at sensible but at considerable distances.

Tension.—The attractions and repulsions at sensible distances, caused by ordinary electricity, are well known to be so powerful in certain cases as to surpass, almost infinitely, the similar phenomena produced by electricity, otherwise excited. But still those attractions and repulsions are exactly of the same nature as those already referred to under the head *Tension, Voltaic electricity;* and the difference in degree between them is not greater than often occurs between cases of ordinary electricity only.

The discharge of common electricity through heated air is a well-known fact. The parallel case of voltaic electricity has already been described.

In motion: i. *Evolution of heat.*—The heating power of common electricity, when passed through wires or other substances, is perfectly well known. The accordance between it and voltaic electricity is in this respect complete.

ii. *Magnetism.*—Voltaic electricity has most extraordinary and exalted magnetic powers. If common electricity be identical with it, it ought to have the same powers. In rendering needles or bars magnetic, it is found to agree with voltaic electricity, and the *direction* of the magnetism, in both cases, is the same; but in deflecting the magnetic needle, common electricity has been found deficient, so that sometimes its power has been denied altogether, and at other times distinctions have been hypothetically assumed for the purpose of avoiding the difficulty.

M. Colladon, of Geneva, considered that the difference might be due to the use of insufficient quantities of common electricity in all the experiments before made on this head; and in a memoir read to the Académie des Sciences in 1826, describes experiments in which, by the use of a battery, points, and a delicate galvanometer, he succeeded in obtaining deflections, and thus establishing identity in that respect. I am happy to say that my results fully confirm those by M. Colladon, and I should have had no occasion to describe them, but that they are essential as proofs of the accuracy of the final and general conclusions I am enabled to draw respecting the magnetic and chemical action of electricity.

The plate electrical machine I have used is fifty inches in diameter; it has two sets of rubbers; its prime conductor consists of two brass cylinders connected by a third, the whole length being twelve feet, and the surface in contact with air about 1422 square inches. When in good excitation, one revolution of the plate will give ten or twelve sparks from the conductors, each an inch in length. Sparks or flashes from ten to fourteen inches in length may easily be drawn from the conductors. Each turn of the machine, when worked moderately, occupies about four fifths of a second.

The electric battery consisted of fifteen equal jars. They are coated eight inches upwards from the bottom, and are twenty-three inches in circumference, so that each contains 184 square inches of glass, coated on both sides; this is independent of the bottoms, which are of thicker glass, and contain each about fifty square inches.

A good *discharging train* was arranged by connecting metallically a sufficiently thick wire with the metallic gas pipes of the house, with the metallic gas pipes belonging to the public gasworks of London, and also with the metallic water pipes of London. It was so effectual in its office as to carry off instantaneously electricity of the feeblest tension, even that of a single voltaic trough, and was essential to many of the experiments.

It was to the retarding power of bad conductors, with the intention of diminishing its *intensity* without altering its *quantity,* that I first

looked with the hope of being able to make common electricity assume more of the characters and power of voltaic electricity than it is usually supposed to have.

The coating and armour of the galvanometer were first connected with the discharging train; the end B (Fig. 3) of the galvanometer wire was connected with the outside coating of the battery, and then both these with the discharging train; the end A of the galvanometer wire was connected with a discharging rod by a wet thread four feet long; and finally, when the battery had been positively charged by about forty turns of the machine, it was discharged by the rod and the thread through the galvanometer. The needle immediately moved.

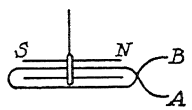

FIG. 3.

During the time that the needle completed its vibration in the first direction and returned, the machine was worked, and the battery recharged; and when the needle in vibrating resumed its first direction, the discharge was again made through the galvanometer. By repeating this action a few times, the vibrations soon extended to above 40° on each side of the line of rest.

This effect could be obtained at pleasure. Nor was it varied, apparently, either in direction or degree, by using a short thick string, or even four short thick strings in place of the long fine thread. With a more delicate galvanometer, an excellent swing of the needle could be obtained by one discharge of the battery.

On reversing the galvanometer communications so as to pass the discharge through from B to A, the needle was equally well deflected, but in the opposite direction.

The deflections were in the same direction as if a voltaic current had been passed through the galvanometer, *i.e.* the positively charged surface of the electric battery coincided with the positive end of the voltaic apparatus, and the negative surface of the former with the negative end of the latter.

The battery was then thrown out of use, and the communications so arranged that the current could be passed from the prime conductor, by the discharging rod held against it, through the wet string, through the galvanometer coil, and into the discharging train, by which it was finally dispersed. This current could be stopped at any moment, by removing the discharging rod, and either stopping the machine or connecting the prime conductor by another rod with the discharging train; and could be as instantly renewed. The needle was so adjusted that, whilst vibrating in moderate and small arcs, it required time equal to twenty-five beats of a watch to pass in one direction through the arc, and of course an equal time to pass in the other direction.

Thus arranged, and the needle being stationary, the current, direct from the machine, was sent through the galvanometer for twenty-five beats, then interrupted for other twenty-five beats, renewed for twenty-five beats more, again interrupted for an equal time, and so on continually. The needle soon began to vibrate visibly, and after several alternations of this kind, the vibration increased to 40° or more.

On changing the direction of the current through the galvanometer, the direction of the deflection of the needle was also changed. In all cases the motion of the needle was in direction the same as that caused either by the use of the electric battery or a voltaic trough.

I now rejected the wet string, and substituted a copper wire, so that the electricity of the machine passed at once into wires communicating directly with the discharging train, the galvanometer coil being one of the wires used for the discharge. The effects were exactly those obtained above.

Instead of passing the electricity through the system, by bringing the discharging rod at the end of it into contact with the conductor, four points were fixed on to the rod; when the current was to pass, they were held about twelve inches from the conductor, and when it was not to pass, they were turned away. Then operating as before, except with this variation, the needle was soon powerfully deflected, and in perfect consistency with the former results. Points afforded the means by which Colladon, in all cases, made his discharges.

Finally, I passed the electricity first through an exhausted receiver, so as to make it there resemble the aurora borealis, and then through the galvanometer to the earth; and it was found still effective in deflecting the needle, and apparently with the same force as before.

From all these experiments, it appears that a current of common electricity, whether transmitted through water or metal, or rarefied air, or by means of points in common air, is still able to deflect the needle; the only requisite being, apparently, to allow time for its action: that it is, in fact, just as magnetic in every respect as a voltaic current, and that in this character therefore no distinction exists.

iii. *Chemical decomposition.*—The chemical action of voltaic electricity is characteristic of that agent, but not more characteristic than are the *laws* under which the bodies evolved by decomposition arrange themselves at the poles. Dr. Wollaston showed that common electricity resembled it in these effects, and "that they are both essentially the same."

I first repeated Wollaston's fourth experiment, in which the ends of coated silver wires are immersed in a drop of sulphate of copper. By passing the electricity of the machine through such an arrangement, that end in the drop which received the electricity became coated with metallic copper. One hundred turns of the machine produced an evident effect; two hundred turns a very sensible one. The decomposing action was, however, very feeble. Very little copper was precipitated, and no sensible trace of silver from the other pole appeared in the solution.

A much more convenient and effectual arrangement for chemical de-

compositions by common electricity is the following. Upon a glass plate, Fig. 4, placed over but raised above a piece of white paper, so that shadows may not interfere, put two pieces of tinfoil *a, b;* connect one of these by an insulated wire *c,* or wire and string, with the machine, and the other, *g,* with the discharging train or the negative conductor; provide two pieces of fine platina wire, bent as in Fig. 5, so that the part *d, f* shall be

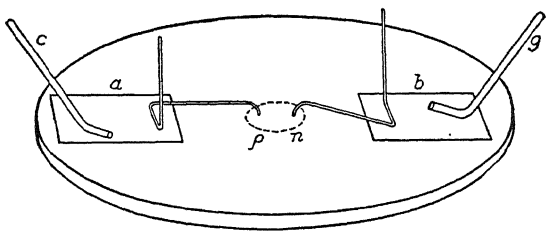

FIG. 4.

nearly upright, whilst the whole is resting on the three bearing points *p, c, f;* place these as in Fig. 4; the points *p, n* then become the decomposing poles. In this way surfaces of contact, as minute as possible, can be obtained at pleasure, and the connection can be broken or renewed in a moment, and the substances acted upon examined with the utmost facility.

A coarse line was made on the glass with solution of sulphate of copper, and the terminations *p* and *n* put into it; the foil *a* was connected

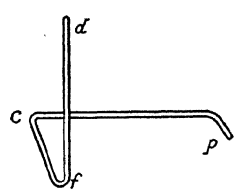

FIG. 5.

with the positive conductor of the machine by wire and wet string, so that no sparks passed: twenty turns of the machine caused the precipitation of so much copper on the end *n* that it looked like copper wire; no apparent change took place at *p*.

On combining a piece of litmus with a piece of turmeric paper, wetting both with solution of sulphate of soda, and putting the paper on the glass, so that *p* was on the litmus and *n* on the turmeric, a very few turns of the machine sufficed to show the evolution of acid at the former and alkali at the latter, exactly in the manner effected by a volta-electric current.

Decompositions took place equally well, whether the electricity passed from the machine to the foil *a,* through water, or through wire

only; by *contact* with the conductor, or by *sparks* there; provided the sparks were not so large as to cause the electricity to pass in sparks from *p* to *n*, or towards *n;* and I have seen no reason to believe that in cases of true electro-chemical decomposition by the machine, the electricity passed in sparks from the conductor, or at any part of the current, is able to do more, because of its tension, than that which is made to pass merely as a regular current.

Finally, the experiment was extended into the following form, supplying in this case the fullest analogy between common and voltaic electricity. Three compound pieces of litmus and turmeric paper were moistened in solution of sulphate of soda, and arranged on a plate of glass with platina wires, as in Fig. 6. The wire *m* was connected with the prime conductor of the machine, the wire *t* with the discharging train,

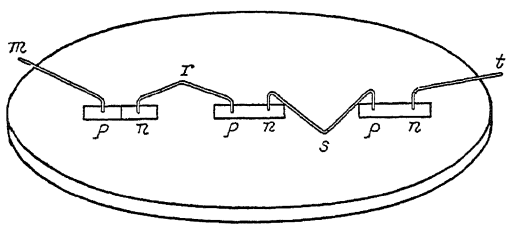

FIG. 6.

and the wires *r* and *s* entered into the course of the electrical current by means of the pieces of moistened paper; they were so bent as to rest each on three points, *n, r, p; n, s, p,* the points *r* and *s* being supported by the glass, and the others by the papers: the three terminations *p, p, p* rested on the litmus, and the other three *n, n, n* on the turmeric paper. On working the machine for a short time only, acid was evolved at *all* the poles or terminations *p, p, p,* by which the electricity entered the solution, and alkali at the other poles *n, n, n,* by which the electricity left the solution.

I have been the more anxious to assign the true value of this experiment as a test of electro-chemical action, because I shall have occasion to refer to it in cases of supposed chemical action by magneto-electric and other electric currents and elsewhere. But, independent of it, there cannot be now a doubt that Dr. Wollaston was right in his general conclusion; and that voltaic and common electricity have powers of chemical decomposition, alike in their nature, and governed by the same law of arrangement.

iv. *Physiological effects.*—The power of the common electric current to shock and convulse the animal system, and when weak to affect the tongue and the eyes, may be considered as the same with the similar power of voltaic electricity, account being taken of the intensity of the one electricity and duration of the other. When a wet thread was interposed in the course of the current of common electricity from the battery charged by eight or ten revolutions of the machine in good action, and the dis-

charge made by platina spatulas through the tongue or the gums, the effect upon the tongue and eyes was exactly that of a momentary feeble voltaic circuit.

v. *Spark.*—The beautiful flash of light attending the discharge of common electricity is well known. It rivals in brilliancy, if it does not even very much surpass, the light from the discharge of voltaic electricity; but it endures for an instant only, and is attended by a sharp noise like that of a small explosion. Still no difficulty can arise in recognising it to be the same spark as that from the voltaic battery, especially under certain circumstances. The eye cannot distinguish the difference between a voltaic and a common electricity spark, if they be taken between amalgamated surfaces of metal, at intervals only, and through the same distance of air.

3. *Magneto-Electricity*

Tension.—The attractions and repulsions due to the tension of ordinary electricity have been well observed with that evolved by magneto-electric induction. M. Pixii, by using an apparatus, clever in its construction and powerful in its action, was able to obtain great divergence of the gold leaves of an electrometer.

In motion: i. *Evolution of heat.*—The current produced by magneto-electric induction can heat a wire in the manner of ordinary electricity. At the British Association of Science at Oxford, in June of the present year, I had the pleasure, in conjunction with Mr. Harris, Professor Daniell, Mr. Duncan, and others, of making an experiment, for which the great magnet in the museum, Mr. Harris's new electrometer and the magneto-electric coil were put in requisition. The latter had been modified in the manner I have elsewhere described, so as to produce an electric spark when its contact with the magnet was made or broken. The terminations of the spiral, adjusted so as to have their contact with each other broken when the spark was to pass, were connected with the wire in the electrometer, and it was found that each time the magnetic contact was made and broken, expansion of the air within the instrument occurred, indicating an increase, at the moment, of the temperature of the wire.

ii. *Magnetism.*—These currents were discovered by their magnetic power.

iii. *Chemical decomposition.*—I have made many endeavours to effect chemical decomposition by magneto-electricity, but unavailingly. The apparatus of M. Pixii already referred to has, however, in the hands of himself and M. Hachette, given decisive chemical results, so as to complete this link in the chain of evidence. Water was decomposed by it, and the oxygen and hydrogen obtained in separate tubes according to the law governing volta-electric and machine-electric decomposition.

iv. *Physiological effects.*—A frog was convulsed in the earliest experiments on these currents. The sensation upon the tongue, and the flash before the eyes, which I at first obtained only in a feeble degree, have

been since exalted by more powerful apparatus, so as to become even disagreeable.

v. *Spark.*—The feeble spark which I first obtained with these currents has been varied and strengthened by Signori Nobili and Antinori, and others, so as to leave no doubt as to its identity with the common electric spark.

4. Thermo-Electricity

With regard to thermo-electricity (that beautiful form of electricity discovered by Seebeck), the very conditions under which it is excited are such as to give no ground for expecting that it can be raised like common electricity to any high degree of tension; the effects, therefore, due to that state are not to be expected. The sum of evidence respecting its analogy to the electricities already described is, I believe, as follows:—*Tension.* The attractions and repulsions due to a certain degree of tension have not been observed. *In currents:* i. *Evolution of heat.* I am not aware that its power of raising temperature has been observed. ii. *Magnetism.* It was discovered, and is best recognised, by its magnetic powers. iii. *Chemical decomposition* has not been effected by it. iv. *Physiological effects.* Nobili has shown that these currents are able to cause contractions in the limbs of a frog. v. *Spark.* The spark has not yet been seen.

Only those effects are weak or deficient which depend upon a certain high degree of intensity; and if common electricity be reduced in that quality to a similar degree with the thermo-electricity, it can produce no effects beyond the latter.

5. Animal Electricity

After an examination of the experiments of Walsh, Ingenhousz, Cavendish, Sir H. Davy, and Dr. Davy, no doubt remains on my mind as to the identity of the electricity of the torpedo with common and voltaic electricity; and I presume that so little will remain on the minds of others as to justify my refraining from entering at length into the philosophical proofs of that identity. At present the sum of evidence is as follows:—

Tension.—No sensible attractions or repulsions due to tension have been observed.

In motion: i. *Evolution of heat;* not yet observed; I have little or no doubt that Harris's electrometer would show it.

ii. *Magnetism.*—Perfectly distinct. According to Dr. Davy, the current deflected the needle and made magnets under the same law, as to direction, which governs currents of ordinary and voltaic electricity.

iii. *Chemical decomposition.*—Also distinct; and though Dr. Davy used an apparatus of similar construction with that of Dr. Wollaston, still no error in the present case is involved, for the decompositions were

polar, and in their nature truly electro-chemical. By the direction of the magnet, it was found that the under surface of the fish was negative, and the upper positive; and in the chemical decompositions, silver and lead were precipitated on the wire connected with the under surface, and not on the other; and when these wires were either steel or silver, in solution of common salt, gas (hydrogen?) rose from the negative wire, but none from the positive.

iv. *Physiological effects.*—These are so characteristic that by them the peculiar powers of the torpedo and gymnotus are principally recognised.

v. *Spark.*—The electric spark has not yet been obtained.

In concluding this summary of the powers of torpedinal electricity, I cannot refrain from pointing out the enormous absolute quantity of electricity which the animal must put in circulation at each effort. It is doubtful whether any common electrical machine has as yet been able to supply electricity sufficient in a reasonable time to cause true electro-chemical decomposition of water, yet the current from the torpedo has done it. The same high proportion is shown by the magnetic effects. These circumstances indicate that the torpedo has power (in the way probably that Cavendish describes) to continue the evolution for a sensible time, so that its successive discharges rather resemble those of a voltaic arrangement, intermitting in its action, than those of a Leyden apparatus, charged and discharged many times in succession. In reality, however, there is *no philosophical difference* between these two cases.

The *general conclusion* which must, I think, be drawn from this collection of facts is that *electricity, whatever may be its source, is identical in its nature.* The phenomena in the five kinds of species quoted differ, not in their character but only in degree; and in that respect vary in proportion to the variable circumstances of *quantity* and *intensity* which can at pleasure be made to change in almost any one of the kinds of electricity as much as it does between one kind and another.

Table of the experimental Effects common to the Electricities derived from different Sources.

	Physiological Effects.	Magnetic Deflection.	Magnets Made.	Spark.	Heating Power.	True Chemical Action.	Attraction and Repulsion.	Discharge by Hot Air.
1. Voltaic electricity	×	×	×	×	×	×	×	×
2. Common electricity	×	×	×	×	×	×	×	×
3. Magneto-electricity	×	×	×	×	×	×	×	
4. Thermo-electricity	×	×	+	+	+	+		
5. Animal electricity	×	×	×	+	+	×		

II. NEW CONDITIONS OF ELECTRO-CHEMICAL DECOMPOSITION

THE TENSION of machine electricity causes it, however small in quantity, to pass through any length of water, solutions, or other substances classing with these as conductors, as fast as it can be produced, and therefore, in relation to quantity, as fast as it could have passed through much shorter portions of the same conducting substance. With the voltaic battery the case is very different, and the passing current of electricity supplied by it suffers serious diminution in any substance, by considerable extension of its length, but especially in such bodies as those mentioned above.

I endeavoured to apply this facility of transmitting the current of electricity through any length of a conductor to an investigation of the transfer of the elements in a decomposing body, in contrary directions, towards the poles. The general form of apparatus used in these experiments has been already described; and also a particular experiment, in which, when a piece of litmus paper and a piece of turmeric paper were combined and moistened in solution of sulphate of soda, the point of the wire from the machine (representing the positive pole) put upon the litmus paper, and the receiving point from the discharging train, representing the negative pole, upon the turmeric paper, a very few turns of the machine sufficed to show the evolution of acid at the former, and alkali at the latter, exactly in the manner effected by a volta-electric current.

The pieces of litmus and turmeric paper were *now* placed each upon a separate plate of glass, and connected by an insulated string four feet long, moistened in the same solution of sulphate of soda: the terminal decomposing wire points were placed upon the papers as before. On working the machine, the same evolution of acid and alkali appeared as in the former instance, and with equal readiness, notwithstanding that the places of their appearance were four feet apart from each other. Finally, a piece of string, seventy feet long, was used. It was insulated in the air by suspenders of silk, so that the electricity passed through its entire length: decomposition took place exactly as in former cases, alkali and acid appearing at the two extremities in their proper places.

The negative point of the discharging train, the turmeric paper, and the string were then removed; the positive point was left resting upon the litmus paper, and the latter touched by a piece of moistened string held in the hand. A few turns of the machine evolved acid at the positive point as freely as before.

These experiments were varied so as to include the action of only one metallic pole, but that not the pole connected with the machine. Turmeric paper was moistened in solution of sulphate of soda, placed upon glass, and connected with the discharging train by a decomposing wire; a

piece of wet string was hung from it, the lower extremity of which was brought opposite a point connected with the positive prime conductor of the machine. The machine was then worked for a few turns, and alkali immediately appeared at the point of the discharging train which rested on the turmeric paper. Corresponding effects took place at the negative conductor of a machine.

These cases are abundantly sufficient to show that electro-chemical decomposition does not depend upon the simultaneous action of two metallic poles, since a single pole might be used, decomposition ensue, and one or other of the elements liberated, pass to the pole, according as it was positive or negative. In considering the course taken by, and the final arrangement of, the other element, I had little doubt that I should find it had receded towards the other extremity, and that the air itself had acted as a pole, an expectation which was fully confirmed in the following manner.

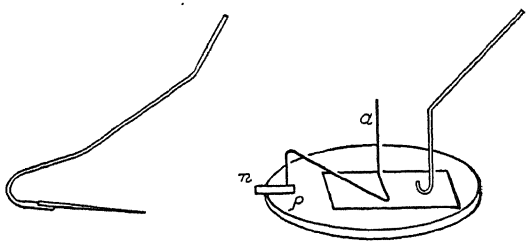

FIG. 7.

A piece of turmeric paper, not more than 0.4 of an inch in length and 0.5 of an inch in width, was moistened with sulphate of soda and placed upon the edge of a glass plate opposite to, and about two inches from, a point connected with the discharging train (Fig. 7); a piece of tinfoil, resting upon the same glass plate, was connected with the machine, and also with the turmeric paper, by a decomposing wire *a*. The machine was then worked, the positive electricity passing into the turmeric paper at the point *p*, and out at the extremity *n*. After forty or fifty turns of the machine, the extremity *n* was examined, and the two points or angles found deeply coloured by the presence of free alkali.

Arrangements were then made in which no metallic communication with the decomposing matter was allowed, but both poles (if they might

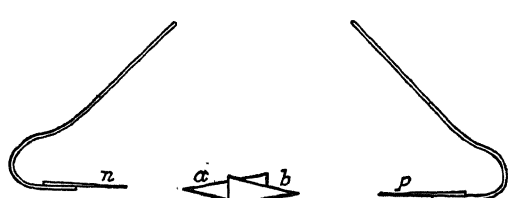

FIG. 8.

now be called by that name) formed of air only. A piece of turmeric paper *a*, Fig. 8, and a piece of litmus paper *b* were dipped in solution of sulphate of soda, put together so as to form one moist pointed conductor, and supported on wax between two needle points, one *p* connected by a wire with the conductor of the machine, and the other, *n*, with the discharging train. The interval in each case between the points was about half an inch: the positive point *p* was opposite the litmus paper; the negative point *n* opposite the turmeric. The machine was then worked for a time, upon which evidence of decomposition quickly appeared, for the point of the litmus *b* became reddened from acid evolved there, and the point of the turmeric *a* red from a similar and simultaneous evolution of alkali.

Upon turning the paper conductor round, so that the litmus point should now give off the positive electricity, and the turmeric point receive it, and working the machine for a short time, both the red spots disappeared, and as on continuing the action of the machine no red spot was re-formed at the litmus extremity, it proved that in the first instance the effect was not due to the action of brushes or mere electric discharges causing the formation of nitric acid from the air.

If the combined litmus and turmeric paper in this experiment be considered as constituting a conductor independent of the machine or the discharging train, and the final places of the elements evolved be considered in relation to this conductor, then it will be found that the acid collects at the *negative* or receiving end or pole of the arrangement, and the alkali at the *positive* or delivering extremity.

Finally, a series of four small compound conductors, consisting of litmus and turmeric paper (Fig. 9) moistened in solution of sulphate of

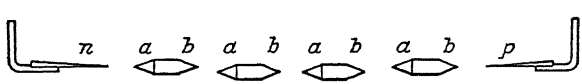

FIG. 9.

soda, were supported on glass rods, in a line at a little distance from each other, between the points *p* and *n* of the machine and discharging train, so that the electricity might pass in succession through them, entering in at the litmus points *b, b* and passing out at the turmeric points *a, a*. On working the machine carefully, so as to avoid sparks and brushes, I soon obtained evidence of decomposition in each of the moist conductors, for all the litmus points exhibited free acid, and the turmeric points equally showed free alkali.

These cases of electro-chemical decomposition are in their nature exactly of the same kind as those affected under ordinary circumstances by the voltaic battery, notwithstanding the great differences as to the presence or absence, or at least as to the nature, of the parts usually called poles; and also of the final situation of the elements eliminated at the electrified boundary surfaces. They indicate at once an internal action of the parts suffering decomposition, and appear to show that the power

which is effectual in separating the elements is exerted there, and not at the poles.

Theory of Electro-chemical Decomposition

The extreme beauty and value of electro-chemical decompositions have given to that power which the voltaic pile possesses of causing their occurrence an interest surpassing that of any other of its properties; for the power is not only intimately connected with the continuance, if not with the production, of the electrical phenomena, but it has furnished us with the most beautiful demonstrations of the nature of many compound bodies; has in the hands of Becquerel been employed in compounding substances; has given us several new combinations, and sustains us with the hope that when thoroughly understood it will produce many more.

What may be considered as the general facts of electro-chemical decomposition are agreed to by nearly all who have written on the subject. They consist in the separation of the decomposable substance acted upon into its proximate or sometimes ultimate principles, whenever both poles of the pile are in contact with that substance in a proper condition; in the evolution of these principles at distant points, *i.e.* at the poles of the pile, where they are either finally set free or enter into union with the substance of the poles; and in the constant determination of the evolved elements or principles to particular poles according to certain well-ascertained laws.

But the views of men of science vary much as to the nature of the action by which these effects are produced; and as it is certain that we shall be better able to apply the power when we really understand the manner in which it operates, this difference of opinion is a strong inducement to further inquiry. I have been led to hope that the following investigations might be considered, not as an increase of that which is doubtful, but a real addition to this branch of knowledge.

That electro-chemical decomposition does not depend upon any direct attraction and repulsion of the poles (meaning thereby the metallic terminations either of the voltaic battery or ordinary electrical machine arrangements) upon the elements in contact with or near to them appeared very evident from the experiments made in air, when the substances evolved did not collect about any poles, but, in obedience to the direction of the current, were evolved, and I would say ejected, at the extremities of the decomposing substance. But notwithstanding the extreme dissimilarity in the character of air and metals, and the almost total difference existing between them as to their mode of conducting electricity and becoming charged with it, it might perhaps still be contended, although quite hypothetically, that the bounding portions of air were now the surfaces or places of attraction, as the metals had been supposed to be before. In illustration of this and other points, I endeavoured to devise an arrangement by which I could decompose a body against a surface of water, as well as against air or metal, and succeeded in doing so unexcep-

tionably in the following manner. As the experiment for very natural reasons requires many precautions to be successful, and will be referred to hereafter in illustration of the views I shall venture to give, I must describe it minutely.

A glass basin (Fig. 10), four inches in diameter and four inches deep, had a division of mica *a* fixed across the upper part so as to descend one inch and a half below the edge, and be perfectly watertight at the sides: a plate of platina *b*, three inches wide, was put into the basin on one side of the division *a*, and retained there by a glass block below, so that any gas produced by it in a future stage of the experiment should not ascend beyond the mica and cause currents in the liquid on that side. A strong solution of sulphate of magnesia was carefully poured without splashing into the basin, until it rose a little above the lower edge of the mica division *a*, great care being taken that the glass or mica on the unoccupied or

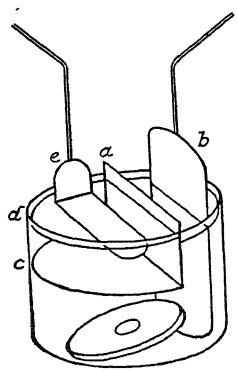

FIG. 10.

c side of the division in the figure should not be moistened by agitation of the solution above the level to which it rose. A thin piece of clean cork, well wetted in distilled water, was then carefully and lightly placed on the solution at the *c* side, and distilled water poured gently onto it until a stratum the eighth of an inch in thickness appeared over the sulphate of magnesia; all was then left for a few minutes, that any solution adhering to the cork might sink away from it, or be removed by the water on which it now floated; and then more distilled water was added in a similar manner, until it reached nearly to the top of the glass. In this way solution of the sulphate occupied the lower part of the glass, and also the upper on the right-hand side of the mica; but on the left-hand side of the division a stratum of water from *c* to *d*, one inch and a half in depth, reposed upon it, the two presenting, when looked through horizontally, a comparatively definite plane of contact. A second platina pole *e* was arranged so as to be just under the surface of the water, in a position nearly horizontal, a little inclination being given to it, that gas evolved during

decomposition might escape: the part immersed was three inches and a half long by one inch wide, and about seven eighths of an inch of water intervened between it and the solution of sulphate of magnesia.

The latter pole *e* was now connected with the negative end of a voltaic battery, of forty pairs of plates four inches square, whilst the former pole *b* was connected with the positive end. There was action and gas evolved at both poles; but from the intervention of the pure water, the decomposition was very feeble compared to what the battery would have effected in a uniform solution. After a little while (less than a minute), magnesia also appeared at the negative side: *it did not make its appearance at the negative metallic pole, but in the water,* at the plane where the solution and the water met; and on looking at it horizontally, it could be there perceived lying in the water upon the solution, not rising more than the fourth of an inch above the latter, whilst the water between it and the negative pole was perfectly clear. On continuing the action, the bubbles of hydrogen rising upwards from the negative pole impressed a circulatory movement on the stratum of water, upwards in the middle, and downwards at the side, which gradually gave an ascending form to the cloud of magnesia in the part just under the pole, having an appearance as if it were there attracted to it; but this was altogether an effect of the currents, and did not occur until long after the phenomena looked for were satisfactorily ascertained.

After a little while the voltaic communication was broken, and the platina poles removed with as little agitation as possible from the water and solution, for the purpose of examining the liquid adhering to them. The pole *e,* when touched by turmeric paper, gave no traces of alkali, nor could anything but pure water be found upon it. The pole *b,* though drawn through a much greater depth and quantity of fluid, was found so acid as to give abundant evidence to litmus paper, the tongue, and other tests. Hence there had been no interference of alkaline salts in any way, undergoing first decomposition, and then causing the separation of the magnesia at a distance from the pole by mere chemical agencies. This experiment was repeated again and again, and always successfully.

As, therefore, the substances evolved in cases of electro-chemical decomposition may be made to appear against air—which, according to common language, is not a conductor, nor is decomposed—or against water, which is a conductor, and can be decomposed—as well as against the metal poles, which are excellent conductors, but undecomposable—there appears but little reason to consider the phenomena generally, as due to the *attraction* or attractive powers of the latter, when used in the ordinary way, since similar attractions can hardly be imagined in the former instances.

If the wires of a galvanometer be terminated by plates, and these be immersed in dilute acid, contained in a regularly formed rectangular glass trough, connected at each end with a voltaic battery by poles equal to the section of the fluid, a part of the electricity will pass through the instrument and cause a certain deflection. And if the plates are always retained

at the *same distance from each other* and from the sides of the trough, are always parallel to each other, and uniformly placed relative to the fluid, then, whether they are immersed near the middle of the decomposing solution or at one end, still the instrument will indicate the same deflection, and consequently the same electric influence.

It is very evident that when the width of the decomposing conductor varies, as is always the case when mere wires or plates, as poles, are dipped into or are surrounded by solution, no constant expression can be given as to the action upon a single particle placed in the course of the current, nor any conclusion of use, relative to the supposed attractive or repulsive force of the poles, be drawn. The force will vary as the distance from the pole varies; as the particle is directly between the poles or more or less on one side; and even as it is nearer to or further from the sides of the containing vessels, or as the shape of the vessel itself varies; and, in fact, by making variations in the form of the arrangement, the force upon any single particle may be made to increase, or diminish, or remain constant, whilst the distance between the particle and the pole shall remain the same; or the force may be made to increase, or diminish, or remain constant, either as the distance increases or as it diminishes.

From numerous experiments, I am led to believe the following general expression to be correct; but I purpose examining it much further, and would therefore wish not to be considered at present as pledged to its accuracy. The *sum of chemical decomposition is constant* for any section taken across a decomposing conductor, uniform in its nature, at whatever distance the poles may be from each other or from the section; or however that section may intersect the currents, whether directly across them or so oblique as to reach almost from pole to pole, or whether it be plane, or curved, or irregular in the utmost degree; provided the current of electricity be retained constant in quantity, and that the section passes through every part of the current through the decomposing conductor.

I have reason to believe that the statement might be made still more general, and expressed thus: That *for a constant quantity of electricity, whatever the decomposing conductor may be, whether water, saline solutions, acids, fused bodies, etc., the amount of electro-chemical action is also a constant quantity,* i.e. *would always be equivalent to a standard chemical effect founded upon ordinary chemical affinity.* I have this investigation in hand, with several others, and shall be prepared to give it in the next part of these *Researches.*

Electro-chemical decomposition is well known to depend essentially upon the *current* of electricity. I have shown that in certain cases the decomposition is proportionate to the quantity of electricity passing, whatever may be its intensity or its source, and that the same is probably true for all cases, even when the utmost generality is taken on the one hand and great precision of expression on the other.

Passing to the consideration of electro-chemical decomposition, it appears to me that the effect is produced by an *internal corpuscular action,*

exerted according to the direction of the electric current, and that it is due to a force either *superadded to* or *giving direction to the ordinary chemical affinity* of the bodies present. The body under decomposition may be considered as a mass of acting particles, all those which are included in the course of the electric current contributing to the final effect; and it is because the ordinary chemical affinity is relieved, weakened, or partly neutralised by the influence of the electric current in one direction parallel to the course of the latter, and strengthened or added to in the opposite direction, that the combining particles have a tendency to pass in opposite courses..

In this view the effect is considered as *essentially dependent* upon the *mutual chemical affinity* of the particles of opposite kinds. Particles *a a,* Fig. 11, could not be transferred or travel from one pole N towards the other P unless they found particles of the opposite kind *b b,* ready to pass in the contrary direction: for it is by virtue of their increased affinity for those particles, combined with their diminished affinity for such as are behind them in their course, that they are urged forward: and when any one particle *a,* Fig. 12, arrives at the pole, it is excluded or set free, be-

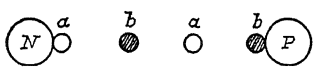

FIG. 11.

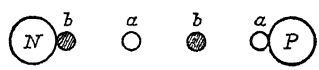

FIG. 12.

cause the particle *b* of the opposite kind, with which it was the moment before in combination, has, under the superinducing influence of the current, a greater attraction for the particle *a,* which is before it in its course, than for the particle *a,* towards which its affinity has been weakened.

As far as regards any single compound particle, the case may be considered as analogous to one of ordinary decomposition, for in Fig. 12, *a* may be conceived to be expelled from the compound *a b* by the superior attraction of *a* for *b,* that superior attraction belonging to it in consequence of the relative position of *a b* and *a* to the direction of the axis of electric power superinduced by the current. But as all the compound particles in the course of the current, except those actually in contact with the poles, act conjointly, and consist of elementary particles, which whilst they are in one direction expelling are in the other being expelled, the case becomes more complicated but not more difficult of comprehension.

It is not here assumed that the acting particles must be in a right line between the poles. The lines of action which may be supposed to represent the electric currents passing through a decomposing liquid have in many experiments very irregular forms; and even in the simplest case of two wires or points immersed as poles in a drop or larger single portion of fluid, these lines must diverge rapidly from the poles; and the direction in which the chemical affinity between particles is most powerfully modified will vary with the direction of these lines, according constantly with them. But even in reference to these lines or currents, it is not supposed that the particles which mutually affect each other must of necessity be

parallel to them, but only that they shall accord generally with their direction. Two particles, placed in a line perpendicular to the electric current passing in any particular place, are not supposed to have their ordinary chemical relations towards each other affected; but as the line joining them is inclined one way to the current their mutual affinity is increased; as it is inclined in the other direction it is diminished; and the effect is a maximum, when that line is parallel to the current.

That the actions, of whatever kind they may be, take place frequently in oblique directions is evident from the circumstance of those particles being included which in numerous cases are not in a line between the poles. Thus, when wires are used as poles in a glass of solution, the decompositions and recompositions occur to the right or left of the direct line between the poles, and indeed in every part to which the currents extend, as is proved by many experiments, and must therefore often occur between particles obliquely placed as respects the current itself; and when a metallic vessel containing the solution is made one pole, whilst a mere point or wire is used for the other, the decompositions and recompositions must frequently be still more oblique to the course of the currents.

I hope I have now distinctly stated, although in general terms, the view I entertain of the cause of electro-chemical decomposition, *as far as that cause can at present be traced and understood.* I conceive the effects to arise from forces which are *internal,* relative to the matter under decomposition,—and not *external,* as they might be considered, if directly dependent upon the poles. I suppose that the effects are due to a modification, by the electric current, of the chemical affinity of the particles through or by which that current is passing, giving them the power of acting more forcibly in one direction than in another, and consequently making them travel by a series of successive decompositions and recompositions in opposite directions, and finally causing their expulsion or exclusion at the boundaries of the body under decomposition, in the direction of the current, *and that* in larger or smaller quantities, according as the current is more or less powerful. I think, therefore, it would be more philosophical, and more directly expressive of the facts, to speak of such a body in relation to the current passing through it, rather than to the poles, as they are usually called, in contact with it; and say that whilst under decomposition, oxygen, chlorine, iodine, acids, etc., are rendered at its negative extremity, and combustibles, metals, alkalies, bases, etc., at its positive extremity. I do not believe that a substance can be transferred in the electric current beyond the point where it ceases to find particles with which it can combine; and I may refer to the experiments made in air, and in water, already quoted, for facts illustrating these views in the first instance.

The theory I have ventured to put forth appears to me to explain all the prominent features of electro-chemical decomposition in a satisfactory manner.

In the first place, it explains why, in all ordinary cases, the evolved substances *appear only at the poles;* for the poles are the limiting surfaces

of the decomposing substance, and except at them, every particle finds other particles having a contrary tendency with which it can combine.

Then it explains why, in numerous cases, the elements or evolved substances are not *retained* by the poles; and this is no small difficulty in those theories which refer the decomposing effect directly to the attractive power of the poles. If, in accordance with the usual theory, a piece of platina be supposed to have sufficient power to attract a particle of hydrogen from the particle of oxygen with which it was the instant before combined, there seems no sufficient reason, nor any fact, except those to be explained, which shows why it should not, according to analogy with all ordinary attractive forces, as those of gravitation, magnetism, cohesion, chemical affinity, etc., *retain* that particle which it had just before taken from a distance and from previous combination. Yet it does not do so, but allows it to escape freely. Nor does this depend upon its assuming the gaseous state, for acids and alkalies, etc., are left equally at liberty to diffuse themselves through the fluid surrounding the pole, and show no particular tendency to combine with or adhere to the latter.

But in the theory that I have just given, the effect appears to be a natural consequence of the action: the evolved substances are *expelled* from the decomposing mass, not *drawn out by an attraction* which ceases to act on one particle without any assignable reason, while it continues to act on another of the same kind: and whether the poles be metal, water, or air, still the substances are evolved, and are sometimes set free, whilst at others they unite to the matter of the poles, according to the chemical nature of the latter, *i.e.* their chemical relation to those particles which are leaving the substance under operation.

The theory accounts for the *transfer of elements* in a manner which seems to me at present to leave nothing unexplained; and it was, indeed, the phenomena of transfer in the numerous cases of decomposition of bodies rendered fluid by heat, which, in conjunction with the experiments in air, led to its construction.

Chloride of silver furnishes a beautiful instance, especially when decomposed by silver-wire poles. Upon fusing a portion of it on a piece of glass, and bringing the poles into contact with it, there is abundance of silver evolved at the negative pole, and an equal abundance absorbed at the positive pole, for no chlorine is set free: and by careful management, the negative wire may be withdrawn from the fused globule as the silver is reduced there, the latter serving as the continuation of the pole, until a wire or thread of revived silver, five or six inches in length, is produced; at the same time the silver at the positive pole is as rapidly dissolved by the chlorine, which seizes upon it, so that the wire has to be continually advanced as it is melted away. The whole experiment includes the action of only two elements, silver and chlorine, and illustrates in a beautiful manner their progress in opposite directions, parallel to the electric current, which is for the time giving a uniform general direction to their mutual affinities.

According to my theory, an element or a substance not decomposable

under the circumstances of the experiment (as, for instance, a dilute acid or alkali) should not be transferred, or pass from pole to pole, unless it be in chemical relation to some other element or substance tending to pass in the opposite direction, for the effect is considered as essentially due to the mutual relation of such particles.

In support of these arguments, it may be observed that as yet no determination of a substance to a pole, or tendency to obey the electric current, has been observed (that I am aware of) in cases of mere mixture; *i.e.* a substance diffused through a fluid, but having no sensible chemical affinity with it or with substances that may be evolved from it during the action, does not in any case seem to be affected by the electric current. Pulverised charcoal was diffused through dilute sulphuric acid, and subjected with the solution to the action of a voltaic battery, terminated by platina poles; but not the slightest tendency of the charcoal to the negative pole could be observed. Sublimed sulphur was diffused through similar acid, and submitted to the same action, a silver plate being used as the negative pole; but the sulphur had no tendency to pass to that pole, the silver was not tarnished, nor did any sulphuretted hydrogen appear. The case of magnesia and water, with those of comminuted metals in certain solutions, is also of this kind; and, in fact, substances which have the instant before been powerfully determined towards the pole, as magnesia from sulphate of magnesia, become entirely *indifferent to it* the moment they assume their independent state, and pass away, diffusing themselves through the surrounding fluid.

It may be expressed as a general consequence that the more directly bodies are opposed to each other in chemical affinity, the more *ready* is their separation from each other in cases of electro-chemical decomposition, *i.e.* provided other circumstances, as insolubility, deficient conducting power, proportions, etc., do not interfere. This is well known to be the case with water and saline solutions; and I have found it to be equally true with *dry* chlorides, iodides, salts, etc., rendered subject to electro-chemical decomposition by fusion. So that in applying the voltaic battery for the purpose of decomposing bodies not yet resolved into forms of matter simpler than their own, it must be remembered that success may depend not upon the weakness, or failure upon the strength, of the affinity by which the elements sought for are held together, but contrariwise; and then modes of application may be devised by which, in *association* with ordinary chemical powers, and the assistance of fusion, we may be able to penetrate much further than at present into the constitution of our chemical elements.

Some of the most beautiful and surprising cases of electro-chemical decomposition and *transfer* which Sir Humphry Davy described in his celebrated paper were those in which acids were passed through alkalies, and alkalies or earths through acids; and the way in which substances having the most powerful attractions for each other were thus prevented from combining, or, as it is said, had their natural affinity destroyed or suspended throughout the whole of the circuit, excited the utmost aston-

ishment. But if I be right in the view I have taken of the effects, it will appear that that which made the *wonder* is in fact the *essential condition* of transfer and decomposition, and that the more alkali there is in the course of an acid, the more will the transfer of that acid be facilitated from pole to pole; and perhaps a better illustration of the difference between the theory I have ventured and those previously existing cannot be offered than the views they respectively give of such facts as these.

III. ELECTRO-CHEMICAL DECOMPOSITION—Continued

THE THEORY which I believe to be a true expression of the facts of electro-chemical decomposition, and which I have therefore detailed in a former part of these *Researches,* is so much at variance with those previously advanced that I find the greatest difficulty in stating results, as I think, correctly, whilst limited to the use of terms which are current with a certain accepted meaning. Of this kind is the term *pole,* with its prefixes of positive and negative, and the attached ideas of attraction and repulsion. The general phraseology is that the positive pole *attracts* oxygen, acids, etc., or, more cautiously, that it *determines* their evolution upon its surface; and that the negative pole acts in an equal manner upon hydrogen, combustibles, metals, and bases. According to my view, the determining force is *not* at the poles, but *within* the body under decomposition; and the oxygen and acids are rendered at the *negative* extremity of that body, whilst hydrogen, metals, etc., are evolved at the *positive* extremity.

To avoid, therefore, confusion and circumlocution, and for the sake of greater precision of expression than I can otherwise obtain, I have deliberately considered the subject with two friends, and with their assistance and concurrence in framing them, I purpose henceforward using certain other terms, which I will now define. The *poles,* as they are usually called, are only the doors or ways by which the electric current passes into and out of the decomposing body; and they, of course, when in contact with that body, are the limits of its extent in the direction of the current. The term has been generally applied to the metal surfaces in contact with the decomposing substance; but whether philosophers generally would also apply it to the surfaces of air and water, against which I have effected electro-chemical decomposition, is subject to doubt. In place of the term pole, I propose using that of *Electrode,* and I mean thereby that substance, or rather surface, whether of air, water, metal, or any other body, which bounds the extent of the decomposing matter in the direction of the electric current.

The surfaces at which, according to common phraseology, the electric current enters and leaves a decomposing body are most important places of action, and require to be distinguished apart from the poles, with which they are mostly, and the electrodes, with which they are always, in contact. Wishing for a natural standard of electric direction to which I might refer these, expressive of their difference and at the same time free

from all theory, I have thought it might be found in the earth. If the magnetism of the earth be due to electric currents passing round it, the latter must be in a constant direction, which, according to present usage of speech, would be from east to west, or, which will strengthen this help to the memory, that in which the sun appears to move. If in any case of electro-decomposition we consider the decomposing body as placed so that the current passing through it shall be in the same direction, and parallel to that supposed to exist in the earth, then the surfaces at which the electricity is passing into and out of the substance would have an invariable reference, and exhibit constantly the same relations of powers. Upon this notion we purpose calling that towards the east the *anode,* and that towards the west the *cathode;* and whatever changes may take place in our views of the nature of electricity and electrical action, as they must affect the *natural standard* referred to, in the same direction, and to an equal amount with any decomposing substances to which these terms may at any time be applied, there seems no reason to expect that they will lead to confusion or tend in any way to support false views. The *anode* is therefore that surface at which the electric current, according to our present expression, enters: it is the *negative* extremity of the decomposing body; is where oxygen, chlorine, acids, etc., are evolved; and is against or opposite the positive electrode. The *cathode* is that surface at which the current leaves the decomposing body, and is its *positive* extremity; the combustible bodies, metals, alkalies, and bases, are evolved there, and it is in contact with the negative electrode.

I shall have occasion in these *Researches,* also, to class bodies together according to certain relations derived from their electrical actions; and wishing to express those relations without at the same time involving the expression of any hypothetical views, I intend using the following names and terms. Many bodies are decomposed directly by the electric current, their elements being set free; these I propose to call *electrolytes.* Water, therefore, is an electrolyte. The bodies which, like nitric or sulphuric acids, are decomposed in a secondary manner are not included under this term. Then, for *electro-chemically decomposed,* I shall often use the term *electrolysed,* derived in the same way, and implying that the body spoken of is separated into its components under the influence of electricity: it is analogous in its sense and sound to *analyse,* which is derived in a similar manner. The term *electrolytical* will be understood at once: muriatic acid is electrolytical, boracic acid is not.

Finally, I require a term to express those bodies which can pass to the *electrodes,* or, as they are usually called, the poles. Substances are frequently spoken of as being *electro-negative* or *electro-positive,* according as they go under the supposed influence of a direct attraction to the positive or negative pole. But these terms are much too significant for the use to which I should have to put them; for though the meanings are perhaps right, they are only hypothetical, and may be wrong; and then, through a very imperceptible, but still very dangerous, because continual, influence, they do great injury to science, by contracting and limiting the habitual

views of those engaged in pursuing it. I propose to distinguish such bodies by calling those *anions* which go to the *anode* of the decomposing body; and those passing to the *cathode, cations;* and when I have occasion to speak of these together, I shall call them *ions*. Thus the chloride of lead is an *electrolyte,* and when *electrolysed* evolves the two *ions,* chlorine and lead, the former being an *anion* and the latter a *cation.*

On a new Measurer of Volta-electricity

I have already said, when introducing my theory of electro-chemical decomposition, that the chemical decomposing action of a current *is constant for a constant quantity of electricity,* notwithstanding the greatest variations in its sources, in its intensity, in the size of the *electrodes* used, in the nature of the conductors (or non-conductors) through which it is passed, or in other circumstances. The conclusive proofs of the truth of these statements shall be given almost immediately.

I endeavoured upon this law to construct an instrument which should measure out the electricity passing through it, and which, being interposed in the course of the current used in any particular experiment, should serve at pleasure, either as a *comparative standard* of effect or as a *positive measurer* of this subtile agent.

There is no substance better fitted, under ordinary circumstances, to be the indicating body in such an instrument than water; for it is decomposed with facility when rendered a better conductor by the addition of acids or salts; its elements may in numerous cases be obtained and collected without any embarrassment from secondary action, and, being gaseous, they are in the best physical condition for separation and measurement.

FIG. 13.

The first precaution needful in the construction of the instrument was to avoid the recombination of the evolved gases, an effect which the positive electrode has been found so capable of producing. For this purpose various forms of decomposing apparatus were used. The first consisted of straight tubes, each containing a plate and wire of platina soldered together by gold, and fixed hermetically in the glass at the closed extremity of the tube (Fig. 13). The tubes were about eight inches long, 0.7 of an inch in diameter, and graduated. The platina plates were about an inch long, as wide as the tubes would permit, and adjusted as near to the mouths of the tubes as was consistent with the safe collection of the gases evolved. In certain cases, where it was required to evolve the elements upon as small a surface as possible, the metallic extremity, instead of being a plate, consisted of the wire bent into the form of a ring (Fig.

FIG. 14.

14). When these tubes were used as measurers, they were filled with the dilute sulphuric acid, inverted in a basin of the same liquid (Fig. 15), and placed in an inclined position, with their mouths near to each other, that as little decomposing matter should intervene as possible; and also in such a direction that the platina plates should be in vertical planes.

FIG. 15.

Another form of apparatus is that delineated (Fig. 16). The tube is bent in the middle; one end is closed; in that end is fixed a wire and plate, *a*, proceeding so far downwards that, when in the position figured, it shall be as near to the angle as possible, consistently with the collection, at the closed extremity of the tube, of all the gas evolved against it. The plane of this plate is also perpendicular. The other metallic termination, *b*, is introduced at the time decomposition is to be effected, being

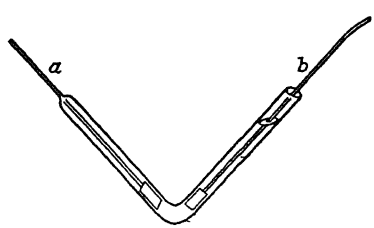

FIG. 16.

brought as near the angle as possible, without causing any gas to pass from it towards the closed end of the instrument. The gas evolved against it is allowed to escape.

The third form of apparatus contains both electrodes in the same tube; the transmission, therefore, of the electricity and the consequent decomposition is far more rapid than in the separate tubes. The resulting gas is the sum of the portions evolved at the two electrodes, and the instrument is better adapted than either of the former as a measurer of the quantity of voltaic electricity transmitted in ordinary cases. It consists of a straight tube (Fig. 17) closed at the upper extremity, and graduated, through the sides of which pass platina wires (being fused into the glass), which are connected with two plates within. The tube is fitted by grinding into one mouth of a double-necked bottle. If the latter be one half or two thirds full of the dilute sulphuric acid, it will, upon inclination of the whole, flow into the tube and fill it. When an electric current is passed through the instrument, the gases evolved against the plates collect in the

upper portion of the tube and are not subject to the recombining power of the platina.

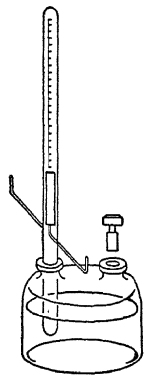

FIG. 17.

Another form of the instrument is given in Fig. 18.

A fifth form is delineated (Fig. 19). This I have found exceedingly useful in experiments continued in succession for days together, and where large quantities of indicating gas were to be collected. It is fixed on a weighted foot, and has the form of a small retort containing the two electrodes: the neck is narrow and sufficiently long to deliver gas issuing from it into a jar placed in a small pneumatic trough. The electrode chamber, sealed hermetically at the part held in the stand, is five inches in length and 0.6 of an inch in diameter; the neck about nine inches in length and 0.4 of an inch in diameter internally. The figure will fully indicate the construction.

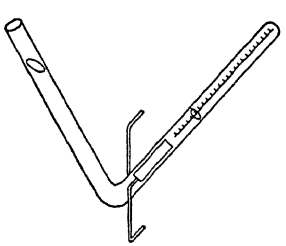

FIG. 18.

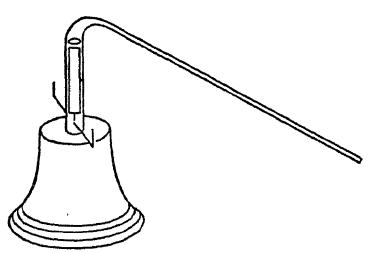

FIG. 19.

Next to the precaution of collecting the gases, if mingled, out of contact with the platina was the necessity of testing the law of a *definite electrolytic* action, upon water at least, under all varieties of condition; that, with a conviction of its certainty, might also be obtained a knowledge of those interfering circumstances which would require to be practically guarded against.

The first point investigated was the influence or indifference of ex-

tensive variations in the size of the electrodes, for which purpose instruments like those last described were used. One of these had plates 0.7 of an inch wide and nearly four inches long; another had plates only 0.5 of an inch wide and 0.8 of an inch long; a third had wires 0.02 of an inch in diameter and three inches long; and a fourth, similar wires only half an inch in length. Yet when these were filled with dilute sulphuric acid, and, being placed in succession, had one common current of electricity passed through them, very nearly the same quantity of gas was evolved in all. The difference was sometimes in favour of one and sometimes on the side of another; but the general result was that the largest quantity of gases was evolved at the smallest electrodes, namely, those consisting merely of platina wires.

Experiments of a similar kind were made with the single-plate straight tubes, and also with the curved tubes, with similar consequences; and when these, with the former tubes, were arranged together in various ways, the result, as to the equality of action of large and small metallic surfaces when delivering and receiving the same current of electricity, was constantly the same. As an illustration, the following numbers are given. An instrument with two wires evolved 74.3 volumes of mixed gases; another with plates, 73.25 volumes; whilst the sum of the oxygen and hydrogen in two separate tubes amounted to 73.65 volumes. In another experiment the volumes were 55.3, 55.3, and 54.4.

But it was observed in these experiments that in single-plate tubes more hydrogen was evolved at the negative electrode than was proportionate to the oxygen at the positive electrode; and generally, also, more than was proportionate to the oxygen and hydrogen in a double-plate tube. Upon more minutely examining these effects, I was led to refer them, and also the differences between wires and plates, to the solubility of the gases evolved, especially at the positive electrode.

With the intention of avoiding this solubility of the gases as much as possible, I arranged the decomposing plates in a vertical position, that the bubbles might quickly escape upwards and that the downward currents in the fluid should not meet ascending currents of gas. This precaution I found to assist greatly in producing constant results, and especially in experiments to be hereafter referred to, in which other liquids than dilute sulphuric acid, as for instance solution of potash, were used.

The irregularities in the indications of the measurer proposed, arising from the solubility just referred to, are but small, and may be very nearly corrected by comparing the results of two or three experiments. They may also be almost entirely avoided by selecting that solution which is found to favour them in the least degree; and still further by collecting the hydrogen only, and using that as the indicating gas; for, being much less soluble than oxygen, being evolved with twice the rapidity and in larger bubbles, it can be collected more perfectly and in greater purity.

From the foregoing and many other experiments, it results that *variation in the size of the electrodes causes no variation in the chemical action of a given quantity of electricity upon water.*

The next point in regard to which the principle of constant electrochemical action was tested was *variation of intensity.* In the first place, the preceding experiments were repeated, using batteries of an *equal* number of plates, *strongly* and *weakly* charged; but the results were alike. They were then repeated, using batteries sometimes containing forty and at other times only five pairs of plates; but the results were still the same. *Variations therefore in the intensity,* caused by difference in the strength of charge or in the number of alternations used, *produced no difference as to the equal action of large and small electrodes.*

The *third point,* in respect to which the principle of equal electrochemical action on water was tested, was *variation of the strength of the solution used.* In order to render the water a conductor, sulphuric acid had been added to it; and it did not seem unlikely that this substance, with many others, might render the water more subject to decomposition, the electricity remaining the same in quantity. But such did not prove to be the case. Diluted sulphuric acid, of different strengths, was introduced into different decomposing apparatus and submitted simultaneously to the action of the same electric current. Slight differences occurred, as before, sometimes in one direction, sometimes in another; but the final result was that *exactly the same quantity of water was decomposed in all the solutions by the same quantity of electricity,* though the sulphuric acid in some was seventyfold what it was in others. The strengths used were of specific gravity 1.495, and downwards.

Although not necessary for the practical use of the instrument I am describing, yet as connected with the important point of constant electrochemical action upon water, I now investigated the effects produced by an electric current passing through aqueous solutions of acids, salts, and compounds, exceedingly different from each other in their nature, and found them to yield astonishingly uniform results. But many of them which are connected with a secondary action will be more usefully described hereafter.

When solutions of caustic potassa or soda, or sulphate of magnesia, or sulphate of soda were acted upon by the electric current, just as much oxygen and hydrogen was evolved from them as from the diluted sulphuric acid, with which they were compared. When a solution of ammonia, rendered a better conductor by sulphate of ammonia, or a solution of subcarbonate of potassa was experimented with, the *hydrogen* evolved was in the same quantity as that set free from the diluted sulphuric acid with which they were compared. Hence *changes in the nature of the solution do not alter the constancy of electrolytic action upon water.*

I consider the foregoing investigation as sufficient to prove the very extraordinary and important principle with respect to WATER, *that when subjected to the influence of the electric current, a quantity of it is decomposed exactly proportionate to the quantity of electricity which has passed,* notwithstanding the thousand variations in the conditions and circumstances under which it may at the time be placed; and further, that when the interference of certain secondary effects, together with the solu-

tion or recombination of the gas and the evolution of air, is guarded against, *the products of the decomposition may be collected with such accuracy as to afford a very excellent and valuable measurer of the electricity concerned in their evolution.*

The forms of instrument which I have given, Figs. 17, 18, 19, are probably those which will be found most useful, as they indicate the quantity of electricity by the largest volume of gases, and cause the least obstruction to the passage of the current. The fluid which my present experience leads me to prefer is a solution of sulphuric acid of specific gravity about 1.336, or from that to 1.25; but it is very essential that there should be no organic substance, nor any vegetable acid, nor other body, which, by being liable to the action of the oxygen or hydrogen evolved at the electrodes, shall diminish their quantity or add other gases to them.

In many cases when the instrument is used as a *comparative standard,* or even as a *measurer,* it may be desirable to collect the hydrogen only, as being less liable to absorption or disappearance in other ways than the oxygen; whilst at the same time its volume is so large as to render it a good and sensible indicator. In such cases the first and second form of apparatus have been used, Figs. 15, 16. The indications obtained were very constant, the variations being much smaller than in those forms of apparatus collecting both gases; and they can also be procured when solutions are used in comparative experiments, which, yielding no oxygen or only secondary results of its action, can give no indications if the educts at both electrodes be collected. Such is the case when solutions of ammonia, muriatic acid, chlorides, iodides, acetates or other vegetable salts, etc., are employed.

In a few cases, as where solutions of metallic salts liable to reduction at the negative electrode are acted upon, the oxygen may be advantageously used as the measuring substance. This is the case, for instance, with sulphate of copper.

There are therefore two general forms of the instrument which I submit as a measurer of electricity; one in which both the gases of the water decomposed are collected, and the other in which a single gas, as the hydrogen only, is used. When referred to as a *comparative instrument* (a use I shall now make of it very extensively), it will not often require particular precaution in the observation; but when used as an *absolute measurer,* it will be needful that the barometric pressure and the temperature be taken into account, and that the graduation of the instruments should be to one scale; the hundredths and smaller divisions of a cubical inch are quite fit for this purpose, and the hundredth may be very conveniently taken as indicating a DEGREE of electricity.

It can scarcely be needful to point out further than has been done how this instrument is to be used. It is to be introduced into the course of the electric current, the action of which is to be exerted anywhere else, and if 60° or 70° of electricity are to be measured out, either in one or several portions, the current, whether strong or weak, is to be continued until the gas in the tube occupies that number of divisions or hundredths

of a cubical inch. Or if a quantity competent to produce a certain effect is to be measured, the effect is to be obtained, and then the indication read off. In exact experiments it is necessary to correct the volume of gas for changes in temperature and pressure, and especially for moisture. For the latter object the volta-electrometer (Fig. 19) is most accurate, as its gas can be measured over water, whilst the others retain it over acid or saline solutions.

I have not hesitated to apply the term *degree* in analogy with the use made of it with respect to another most important imponderable agent, namely, heat; and as the definite expansion of air, water, mercury, etc., is there made use of to measure heat, so the equally definite evolution of gases is here turned to a similar use for electricity.

The instrument offers the only *actual measurer* of voltaic electricity which we at present possess. For without being at all affected by variations in time or intensity, or alterations in the current itself, of any kind, or from any cause, or even of intermissions of action, it takes note with accuracy of the quantity of electricity which has passed through it, and reveals that quantity by inspection; I have therefore named it a VOLTA-ELECTROMETER.

On the primary or secondary character of the bodies evolved at the Electrodes

Before the *volta-electrometer* could be employed in determining, as a *general law,* the constancy of electro-decomposition, it became necessary to examine a distinction, already recognised among scientific men, relative to the products of that action, namely, their primary or secondary character; and, if possible, by some general rule or principle, to decide when they were of the one or the other kind. It will appear hereafter that great mistakes respecting electro-chemical action and its consequences have arisen from confounding these two classes of results together.

When a substance under decomposition yields at the electrodes those bodies uncombined and unaltered which the electric current has separated, then they may be considered as primary results, even though themselves compounds. Thus the oxygen and hydrogen from water are primary results; and so also are the acid and alkali (themselves compound bodies) evolved from sulphate of soda. But when the substances separated by the current are changed at the electrodes before their appearance, then they give rise to secondary results, although in many cases the bodies evolved are elementary.

These secondary results occur in two ways, being sometimes due to the mutual action of the evolved substance and the matter of the electrode, and sometimes to its action upon the substances contained in the body itself under decomposition. Thus, when carbon is made the positive electrode in dilute sulphuric acid, carbonic oxide and carbonic acid occasionally appear there instead of oxygen; for the latter, acting upon the matter of the electrode, produces these secondary results. Or if the posi-

tive electrode, in a solution of nitrate or acetate of lead, be platina, then peroxide of lead appears there, equally a secondary result with the former, but now depending upon an action of the oxygen on a substance in the solution. Again, when ammonia is decomposed by platina electrodes, nitrogen appears at the *anode;* but though an *elementary* body, it is a *secondary* result in this case, being derived from the chemical action of the oxygen electrically evolved there upon the ammonia in the surrounding solution. In the same manner, when aqueous solutions of metallic salts are decomposed by the current, the metals evolved at the *cathode,* though elements, are *always* secondary results, and not immediate consequences of the decomposing power of the electric current.

But when we take to our assistance the law of *constant electro-chemical action* already proved with regard to water, and which I hope to extend satisfactorily to all bodies, and consider the *quantities* as well as the *nature* of the substances set free, a generally accurate judgment of the primary or secondary character of the results may be formed: and this important point, so essential to the theory of electrolysation, since it decides what are the particles directly under the influence of the current (distinguishing them from such as are not affected) and what are the results to be expected, may be established with such degree of certainty as to remove innumerable ambiguities and doubtful considerations from this branch of the science.

Let us apply these principles to the case of ammonia, and the supposed determination of nitrogen to one or the other *electrode.* A pure strong solution of ammonia is as bad a conductor, and therefore as little liable to electrolysation, as pure water; but when sulphate of ammonia is dissolved in it, the whole becomes a conductor; nitrogen *almost* and occasionally *quite* pure is evolved at the *anode,* and hydrogen at the *cathode;* the ratio of the volume of the former to that of the latter varying, but being as 1 to about 3 or 4. This result would seem at first to imply that the electric current had decomposed ammonia, and that the nitrogen had been determined towards the positive electrode. But when the electricity used was measured out by the volta-electrometer, it was found that the hydrogen obtained was exactly in the proportion which would have been supplied by decomposed water, whilst the nitrogen had no certain or constant relation whatever. When, upon multiplying experiments, it was found that, by using a stronger or weaker solution, or a more or less powerful battery, the gas evolved at the *anode* was a mixture of oxygen and nitrogen, varying both in proportion and absolute quantity, whilst the hydrogen at the *cathode* remained constant, no doubt could be entertained that the nitrogen at the *anode* was a secondary result, depending upon the chemical action of the nascent oxygen, determined to that surface by the electric current, upon the ammonia in solution. It was the water, therefore, which was electrolysed, not the ammonia.

I have experimented upon many bodies, with a view to determine whether the results were primary or secondary. I have been surprised to find how many of them, in ordinary cases, are of the latter class, and how

frequently water is the only body electrolysed in instances where other substances have been supposed to give way. Some of these results I will give in as few words as possible.

Nitric acid.—When very strong, it conducted well, and yielded oxygen at the positive electrode. No gas appeared at the negative electrode; but nitrous acid, and apparently nitric oxide, was formed there, which, dissolving, rendered the acid yellow or red, and at last even effervescent, from the spontaneous separation of nitric oxide. Upon diluting the acid with its bulk or more of water, gas appeared at the negative electrode. Its quantity could be varied by variations, either in the strength of the acid or of the voltaic current: for that acid from which no gas separated at the *cathode,* with a weak voltaic battery, did evolve gas there with a stronger; and that battery which evolved no gas there with a strong acid, did cause its evolution with an acid more dilute. The gas at the *anode* was always oxygen; that at the *cathode* hydrogen. When the quantity of products was examined by the volta-electrometer, the oxygen, whether from strong or weak acid, proved to be in the same proportion as from water. When the acid was diluted to specific gravity 1.24, or less, the hydrogen also proved to be the same in quantity as from water. Hence I conclude that the nitric acid does not undergo electrolysation, but the water only; that the oxygen at the *anode* is always a primary result, but that the products at the *cathode* are often secondary, and due to the reaction of the hydrogen upon the nitric acid.

Nitre.—A solution of this salt yields very variable results, according as one or other form of tube is used, or as the electrodes are large or small. Sometimes the whole of the hydrogen of the water decomposed may be obtained at the negative electrode; at other times, only a part of it, because of the ready formation of secondary results. The solution is a very excellent conductor of electricity.

Muriatic acid.—A strong solution gave hydrogen at the negative electrode and chlorine only at the positive electrode; of the latter, a part acted on the platina and a part was dissolved. A minute bubble of gas remained; it was not oxygen, but probably air previously held in solution.

It was an important matter to determine whether the chlorine was a primary result or only a secondary product, due to the action of the oxygen evolved from water at the *anode* upon the muriatic acid; *i.e.* whether the muriatic acid was electrolysable, and, if so, whether the decomposition was *definite.*

The muriatic acid was gradually diluted. One part with six of water gave only chlorine at the *anode.* One part with eight of water gave only chlorine; with nine of water, a little oxygen appeared with the chlorine: but the occurrence or non-occurrence of oxygen at these strengths depended, in part, on the strength of the voltaic battery used. With fifteen parts of water, a little oxygen, with much chlorine, was evolved at the *anode.* As the solution was now becoming a bad conductor of electricity, sulphuric acid was added to it: this caused more ready decomposition, but did not sensibly alter the proportion of chlorine and oxygen.

The muriatic acid was now diluted with 100 times its volume of dilute sulphuric acid. It still gave a large proportion of chlorine at the *anode,* mingled with oxygen; and the result was the same, whether a voltaic battery of forty pairs of plates or one containing only five pairs were used. With acid of this strength, the oxygen evolved at the *anode* was to the hydrogen at the *cathode,* in volume, as 17 is to 64; and therefore the chlorine would have been thirty volumes had it not been dissolved by the fluid.

Next with respect to the quantity of elements evolved. On using the volta-electrometer, it was found that, whether the strongest or the weakest muriatic acid were used, whether chlorine alone or chlorine mingled with oxygen appeared at the *anode,* still the hydrogen evolved at the *cathode* was a constant quantity, *i.e.* exactly the same as the hydrogen which the *same quantity of electricity* could evolve from water.

This constancy does not decide whether the muriatic acid is electrolysed or not, although it proves that if so, it must be in definite proportions to the quantity of electricity used. Other considerations may, however, be allowed to decide the point. The analogy between chlorine and oxygen, in their relations to hydrogen, is so strong as to lead almost to the certainty that, when combined with that element, they would perform similar parts in the process of electro-decomposition. They both unite with it in single proportional or equivalent quantities. In other binary compounds of chlorine also, where nothing equivocal depending on the simultaneous presence of it and oxygen is involved, the chlorine is directly eliminated at the *anode* by the electric current. Such is the case with the chloride of lead, which may be justly compared with protoxide of lead, and stands in the same relation to it as muriatic acid to water. The chlorides of potassium, sodium, barium, etc., are in the same relation to the protoxides of the same metals and present the same results under the influence of the electric current.

From all the experiments, combined with these considerations, I conclude that muriatic acid is decomposed by the direct influence of the electric current, and that the quantities evolved are, and therefore the chemical action is, *definite for a definite quantity of electricity.* For though I have not collected and measured the chlorine, in its separate state, at the *anode,* there can exist no doubt as to its being proportional to the hydrogen at the *cathode;* and the results are therefore sufficient to establish the general law of *constant electro-chemical action* in the case of muriatic acid.

In the dilute acid, I conclude that a part of the water is electrochemically decomposed, giving origin to the oxygen, which appears mingled with the chlorine at the *anode.* The oxygen *may* be viewed as a secondary result; but I incline to believe that it is not so: for, if it were, it might be expected in largest proportion from the stronger acid, whereas the reverse is the fact. This consideration, with others, also leads me to conclude that muriatic acid is more easily decomposed by the electric current than water; since, even when diluted with eight or nine times its

quantity of the latter fluid, it alone gives way, the water remaining unaffected.

Chlorides.—On using solutions of chlorides in water—for instance, the chlorides of sodium or calcium—there was evolution of chlorine only at the positive electrode, and of hydrogen, with the oxide of the base, as soda or lime, at the negative electrode. The process of decomposition may be viewed as proceeding in two or three ways, all terminating in the same results. Perhaps the simplest is to consider the chloride as the substance electrolysed, its chlorine being determined to and evolved at the *anode,* and its metal passing to the *cathode,* where, finding no more chlorine, it acts upon the water, producing hydrogen and an oxide as secondary results. As the discussion would detain me from more important matter, and is not of immediate consequence, I shall defer it for the present. It is, however, of *great consequence* to state that, on using the volta-electrometer, the hydrogen in both cases was definite; and if the results do not prove the definite decomposition of chlorides (which shall be proved elsewhere), they are not in the slightest degree opposed to such a conclusion and do support the *general law.*

Tartaric acid.—Pure solution of tartaric acid is almost as bad a conductor as pure water. On adding sulphuric acid, it conducted well, the results at the positive electrode being primary or secondary in different proportions, according to variations in the strength of the acid and the power of the electric current. Alkaline tartrates gave a large proportion of secondary results at the positive electrode. The hydrogen at the negative electrode remained constant unless certain triple metallic salts were used.

Solutions, of salts containing other vegetable acids, as the benzoates; of sugar, gum, etc., dissolved in dilute sulphuric acid; of resin, albumen, etc., dissolved in alkalies, were in turn submitted to the electrolytic power of the voltaic current. In all these cases, secondary results to a greater or smaller extent were produced at the positive electrode.

In concluding this division of these *Researches,* it cannot but occur to the mind that the final result of the action of the electric current upon substances placed between the electrodes, instead of being simple, may be very complicated. There are two modes by which these substances may be decomposed, either by the direct force of the electric current or by the action of bodies which that current may evolve. There are also two modes by which new compounds may be formed, *i.e.* by combination of the evolving substances, whilst in their nascent state, directly with the matter of the electrode; or else their combination with those bodies which, being contained in, or associated with, the body suffering decomposition, are necessarily present at the *anode* and *cathode.* The complexity is rendered still greater by the circumstance that two or more of these actions may occur simultaneously, and also in variable proportions to each other. But it may in a great measure be resolved by attention to the principles already laid down.

On the definite nature and extent of Electro-chemical Decomposition

In the first part of these *Researches,* after proving the identity of electricities derived from different sources, I announced a law, derived from experiment, which seemed to me of the utmost importance to the science of electricity in general, and that branch of it denominated electro-chemistry in particular. The law was expressed thus: *The chemical power of a current of electricity is in direct proportion to the absolute quantity of electricity which passes.*

It is now my object to consider this great principle more closely, and to develop some of the consequences to which it leads. That the evidence for it may be the more distinct and applicable, I shall quote cases of decomposition subject to as few interferences from secondary results as possible, effected upon bodies very simple yet very definite in their nature.

In the first place, I consider the law as so fully established with respect to the decomposition of *water,* and under so many circumstances which might be supposed, if anything could, to exert an influence over it, that I may be excused entering into further detail respecting that substance, or even summing up the results here.

In the next place, I also consider the law as established with respect to *muriatic acid* by the experiments and reasoning already advanced, when speaking of that substance, in the subdivision respecting primary and secondary results.

Without speaking with the same confidence, yet from the experiments described, and many others not described, relating to hydro-fluoric, hydro-cyanic, ferro-cyanic, and sulpho-cyanic acids, and from the close analogy which holds between these bodies and the hydracids of chlorine, iodine, bromine, etc., I consider these also as coming under subjection to the law and assisting to prove its truth.

FIG. 20.

In the preceding cases, except the first, the water is believed to be inactive; but to avoid any ambiguity arising from its presence, I sought for substances from which it should be absent altogether; and I soon found abundance, amongst which *protochloride of tin* was first subjected to decomposition in the following manner. A piece of platina wire had one extremity coiled up into a small knob, and, having been carefully weighed, was sealed hermetically into a piece of bottle-glass tube, so that the knob should be at the bottom of the tube within (Fig. 20). The tube was suspended by a piece of platina wire, so that the heat of a spirit lamp could be applied to it. Recently fused protochloride of tin was introduced in sufficient quantity to occupy, when melted, about one half of the tube; the wire of the tube was connected with a volta-electrometer, which was itself connected with

the negative end of a voltaic battery; and a platina wire connected with the positive end of the same battery was dipped into the fused chloride in the tube; being, however, so bent that it could not by any shake of the hand or apparatus touch the negative electrode at the bottom of the vessel. The whole arrangement is delineated in Fig. 21.

Under these circumstances the chloride of tin was decomposed: the chlorine evolved at the positive electrode formed bichloride of tin, which passed away in fumes, and the tin evolved at the negative electrode combined with the platina, forming an alloy, fusible at the temperature to which the tube was subjected, and therefore never occasioning metallic communication through the decomposing chloride. When the experiment had been continued so long as to yield a reasonable quantity of gas in the volta-electrometer, the battery connection was broken, the positive electrode removed, and the tube and remaining chloride allowed to cool. When cold, the tube was broken open, the rest of the chloride and the glass being easily separable from the platina wire and its button of alloy. The latter when washed was then reweighed, and the increase gave the weight of the tin reduced.

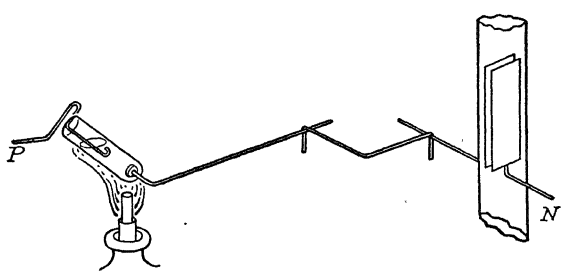

FIG. 21.

I will give the particular results of one experiment, in illustration of the mode adopted in this and others, the results of which I shall have occasion to quote. The negative electrode weighed at first 20 grains; after the experiment, it, with its button of alloy, weighed 23.2 grains. The tin evolved by the electric current at the *cathode* weighed therefore 3.2 grains. The quantity of oxygen and hydrogen collected in the volta-electrometer = 3.85 cubic inches. As 100 cubic inches of oxygen and hydrogen, in the proportions to form water, may be considered as weighing 12.92 grains, the 3.85 cubic inches would weigh 0.49742 of a grain; that being, therefore, the weight of water decomposed by the same electric current as was able to decompose such weight of protochloride of tin as could yield 3.2 grains of metal. Now 0.49742 : 3.2 : : 9 the equivalent of water is to 57.9, which should therefore be the equivalent of tin, if the experiment had been made without error, and if the electro-chemical decomposition *is in this case also definite*. In some chemical works 58 is given as the chemical equivalent of tin, in others 57.9. Both are so near to the result of the

experiment, and the experiment itself is so subject to slight causes of variation (as from the absorption of gas in the volta-electrometer), that the numbers leave little doubt of the applicability of the *law of definite action* in this and all similar cases of electro-decomposition.

It is not often I have obtained an accordance in numbers so near as that I have just quoted. Four experiments were made on the protochloride of tin, the quantities of gas evolved in the volta-electrometer being from 2.05 to 10.29 cubic inches. The average of the four experiments gave 58.53 as the electro-chemical equivalent for tin.

The chloride remaining after the experiment was pure protochloride of tin; and no one can doubt for a moment that the equivalent of chlorine had been evolved at the *anode,* and, having formed bichloride of tin as a secondary result, had passed away.

Chloride of lead was experimented upon in a manner exactly similar, except that a change was made in the nature of the positive electrode; for as the chlorine evolved at the *anode* forms no perchloride of lead, but acts directly upon the platina, it produces, if that metal be used, a solution of chloride of platina in the chloride of lead; in consequence of which a portion of platina can pass to the *cathode,* and would then produce a vitiated result. I therefore sought for, and found in plumbago, another substance, which could be used safely as the positive electrode in such bodies as chlorides, iodides, etc. The chlorine or iodine does not act upon it, but is evolved in the free state; and the plumbago has no reaction, under the circumstances, upon the fused chloride or iodide in which it is plunged. Even if a few particles of plumbago should separate by the heat or the mechanical action of the evolved gas, they can do no harm in the chloride.

The mean of three experiments gave the number of 100.85 as the equivalent for lead. The chemical equivalent is 103.5. The deficiency in my experiments I attribute to the solution of part of the gas in the volta-electrometer; but the results leave no doubt on my mind that both the lead and the chlorine are, in this case, evolved in *definite quantities* by the action of a given quantity of electricity.

I endeavoured to experiment upon the *oxide of lead* obtained by fusion and ignition of the nitrate in a platina crucible, but found great difficulty, from the high temperature required for perfect fusion, and the powerful fluxing qualities of the substance. Green-glass tubes repeatedly failed. I at last fused the oxide in a small porcelain crucible, heated fully in a charcoal fire; and, as it was essential that the evolution of the lead at the *cathode* should take place beneath the surface, the negative electrode was guarded by a green-glass tube, fused around it in such a manner as to expose only the knob of platina at the lower end (Fig. 22), so that it could be plunged beneath the surface, and thus exclude contact of air or oxygen with the lead reduced there. A platina wire was employed for the positive electrode, that metal not being subject to any action from the oxygen evolved against it. The arrangement is given in Fig. 23.

In an experiment of this kind the equivalent for the lead came out

93.17, which is very much too small. This, I believe, was because of the small interval between the positive and negative electrodes in the oxide of lead; so that it was not unlikely that some of the froth and bubbles formed by the oxygen at the *anode* should occasionally even touch the lead reduced at the *cathode,* and re-oxidise it. When I endeavoured to correct this by having more litharge, the greater heat required to keep it all fluid caused a quicker action on the crucible, which was soon eaten through, and the experiment stopped.

In one experiment of this kind I used borate of lead. It evolves lead, under the influence of the electric current, at the *anode,* and oxygen at the *cathode;* and as the boracic acid is not either directly or incidentally decomposed during the operation, I expected a result dependent on the oxide of lead. The borate is not so violent a flux as the oxide, but it

FIG. 22.

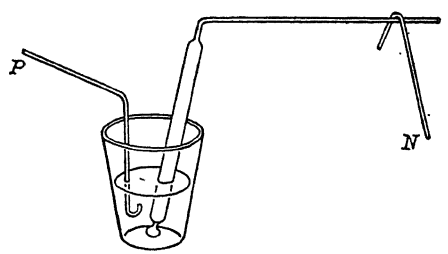

FIG. 23.

requires a higher temperature to make it quite liquid; and if not very hot, the bubbles of oxygen cling to the positive electrode and retard the transfer of electricity. The number for lead came out 101.29, which is so near to 103.5 as to show that the action of the current had been definite.

Iodide of potassium was subjected to electrolytic action in a tube. The negative electrode was a globule of lead, and I hoped in this way to retain the potassium, and obtain results that could be weighed and compared with the volta-electrometer indication; but the difficulties dependent upon the high temperature required, the action upon the glass, the fusibility of the platina induced by the presence of the lead, and other circumstances, prevented me from procuring such results. The iodide was decomposed with the evolution of iodine at the *anode,* and of potassium at the *cathode,* as in former cases.

In some of these experiments several substances were placed in succession, and decomposed simultaneously by the same electric current: thus, protochloride of tin, chloride of lead, and water were thus acted on at once. It is needless to say that the results were comparable, the tin, lead, chlorine, oxygen, and hydrogen evolved being *definite in quantity* and electro-chemical equivalents to each other.

Let us turn to another kind of proof of the *definite chemical action of electricity.* If any circumstances could be supposed to exert an influence

over the quantity of the matters evolved during electrolytic action, one would expect them to be present when electrodes of different substances, and possessing very different chemical affinities for such matters, were used. Platina has no power in dilute sulphuric acid of combining with the oxygen at the *anode,* though the latter be evolved in the nascent state against it. Copper, on the other hand, immediately unites with the oxygen, as the electric current sets it free from the hydrogen; and zinc is not only able to combine with it, but can, without any help from the electricity, abstract it directly from the water, at the same time setting torrents of hydrogen free. Yet in cases where these three substances were used as the positive electrodes in three similar portions of the same dilute sulphuric acid, specific gravity 1.336, precisely the same quantity of water was decomposed by the electric current, and precisely the same quantity of hydrogen set free at the *cathodes* of the three solutions.

The experiment was made thus. Portions of the dilute sulphuric acid were put into three basins. Three volta-electrometer tubes, of the form Figs. 13, 15, were filled with the same acid, and one inverted in each basin. A zinc plate, connected with the positive end of a voltaic battery, was dipped into the first basin, forming the positive electrode there, the hydrogen, which was abundantly evolved from it by the direct action of the acid, being allowed to escape. A copper plate, which dipped into the acid of the second basin, was connected with the negative electrode of the *first* basin; and a platina plate, which dipped into the acid of the third basin, was connected with the negative electrode of the *second* basin. The negative electrode of the third basin was connected with a volta-electrometer, and that with the negative end of the voltaic battery.

Immediately that the circuit was complete, the *electro-chemical action* commenced in all the vessels. The hydrogen still rose in, apparently, undiminished quantities from the positive zinc electrode in the first basin. No oxygen was evolved at the positive copper electrode in the second basin, but a sulphate of copper was formed there; whilst in the third basin the positive platina electrode evolved pure oxygen gas and was itself unaffected. But in *all* the basins the hydrogen liberated at the *negative* platina electrodes was the *same in quantity,* and the same with the volume of hydrogen evolved in the volta-electrometer, showing that in all the vessels the current had decomposed an equal quantity of water. In this trying case, therefore, the *chemical action of electricity* proved to be *perfectly definite.*

A similar experiment was made with muriatic acid diluted with its bulk of water. The three positive electrodes were zinc, silver, and platina; the first being able to separate and combine with the chlorine *without* the aid of the current; the second combining with the chlorine only after the current had set it free; and the third rejecting almost the whole of it. The three negative electrodes were, as before, platina plates fixed within glass tubes. In this experiment, as in the former, the quantity of hydrogen evolved at the *cathodes* was the same for all, and the same as the hydrogen evolved in the volta-electrometer. I have already given my reasons for

believing that in these experiments it is the muriatic acid which is directly decomposed by the electricity; and the results prove that the quantities so decomposed are *perfectly definite* and proportionate to the quantity of electricity which has passed.

Experiments of a similar kind were then made with bodies altogether in a different state, *i.e.* with *fused* chlorides, iodides, etc. I have already described an experiment with fused chloride of silver, in which the electrodes were of metallic silver, the one rendered negative becoming increased and lengthened by the addition of metal, whilst the other was dissolved and eaten away by its abstraction. This experiment was repeated, two weighed pieces of silver wire being used as the electrodes, and a volta-electrometer included in the circuit. Great care was taken to withdraw the negative electrode so regularly and steadily that the crystals of reduced silver should not form a *metallic* communication beneath the surface of the fused chloride. On concluding the experiment the positive electrode was reweighed, and its loss ascertained. The mixture of chloride of silver and metal, withdrawn in successive portions at the negative electrode, was digested in solution of ammonia, to remove the chloride, and the metallic silver remaining also weighed: it was the reduction at the *cathode,* and exactly equalled the solution at the *anode;* and each portion was as nearly as possible the equivalent to the water decomposed in the volta-electrometer.

The infusible condition of the silver at the temperature used and the length and ramifying character of its crystals render the above experiment

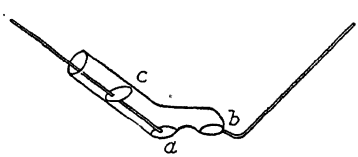

FIG. 24.

difficult to perform and uncertain in its results. I therefore wrought with chloride of lead, using a green-glass tube formed as in Fig. 24. A weighed platina wire was fused into the bottom of a small tube, as before described. The tube was then bent to an angle, at about half an inch distance from the closed end; and the part between the angle and the extremity, being softened, was forced upward, as in the figure, so as to form a bridge, or rather separation, producing two little depressions or basins, *a, b,* within the tube. This arrangement was suspended by a platina wire, as before, so that the heat of a spirit lamp could be applied to it, such inclination being given to it as would allow all air to escape during the fusion of the chloride of lead. A positive electrode was then provided, by bending up the end of a platina wire into a knot, and fusing about twenty grains of metallic lead onto it, in a small closed tube of glass, which was afterwards broken away. Being so furnished, the wire with its lead was weighed, and the weight recorded.

Chloride of lead was now introduced into the tube and carefully fused. The leaded electrode was also introduced; after which the metal, at its extremity, soon melted. In this state of things the tube was filled up to *c* with melted chloride of lead; the end of the electrode to be rendered negative was in the basin *b,* and the electrode of melted lead was retained in the basin *a,* and, by connection with the proper conducting wire of a voltaic battery, was rendered positive. A volta-electrometer was included in the circuit.

Immediately upon the completion of the communication with the voltaic battery, the current passed, and decomposition proceeded. No chlorine was evolved at the positive electrode; but as the fused chloride was transparent, a button of alloy could be observed gradually forming and increasing in size at *b,* whilst the lead at *a* could also be seen gradually to diminish. After a time, the experiment was stopped; the tube allowed to cool, and broken open; the wires, with their buttons, cleaned and weighed; and their change in weight compared with the indication of the volta-electrometer.

In this experiment the positive electrode had lost just as much lead as the negative one had gained, and the loss and gain were very nearly the equivalents of the water decomposed in the volta-electrometer, giving for lead the number 101.5. It is therefore evident, in this instance, that causing *strong affinity,* or *no affinity,* for the substance evolved at the *anode,* to be active during the experiment, produces no variation in the definite action of the electric current.

Then protochloride of tin was subjected to the electric current in the same manner, using, of course, a tin positive electrode. No bichloride of tin was now formed. On examining the two electrodes, the positive had lost precisely as much as the negative had gained; and by comparison with the volta-electrometer, the number for tin came out 59.

All these facts combine into, I think, an irresistible mass of evidence, proving the truth of the important proposition which I at first laid down, namely, *that the chemical power of a current of electricity is in direct proportion to the absolute quantity of electricity which passes.* They prove, too, that this is not merely true with one substance, as water, but generally with all electrolytic bodies; and, further, that the results obtained with any *one substance* do not merely agree amongst themselves, but also with those obtained from *other substances,* the whole combining together into *one series of definite electro-chemical actions.*

The doctrine of *definite electro-chemical action* just laid down, and, I believe, established, leads to some new views of the relations and classifications of bodies associated with or subject to this action. Some of these I shall proceed to consider.

In the first place, compound bodies may be separated into two great classes, namely, those which are decomposable by the electric current and those which are not: of the latter, some are conductors, others non-conductors, of voltaic electricity. The former do not depend for their decomposability upon the nature of their elements only; for, of the same

two elements, bodies may be formed of which one shall belong to one class and another to the other class; but probably on the proportions also. I propose to call bodies of this, the decomposable class, *Electrolytes.*

Then, again, the substances into which these divide, under the influence of the electric current, form an exceedingly important general class. They are combining bodies; are directly associated with the fundamental parts of the doctrine of chemical affinity; and have each a definite proportion, in which they are always evolved during electrolytic action. I have proposed to call these bodies generally *ions,* or particularly *anions* and *cations,* according as they appear at the *anode* or *cathode;* and the numbers representing the proportions in which they are evolved, *electro-chemical equivalents.* Thus hydrogen, oxygen, chlorine, iodine, lead, tin are *ions;* the three former are *anions,* the two metals are *cations,* and 1, 8, 36, 125, 104, 58 are their *electro-chemical equivalents* nearly.

A summary of certain points already ascertained respecting *electrolytes, ions,* and *electro-chemical equivalents* may be given in the following general form of propositions, without, I hope, including any serious error.

i. A single *ion, i.e.* one not in combination with another, will have no tendency to pass to either of the electrodes, and will be perfectly indifferent to the passing current, unless it be itself a compound of more elementary *ions,* and so subject to actual decomposition. Upon this fact is founded much of the proof adduced in favour of the new theory of electro-chemical decomposition, which I put forth in a former part of these *Researches.*

ii. If one *ion* be combined in right proportions with another strongly opposed to it in its ordinary chemical relations, *i.e.* if an *anion* be combined with a *cation,* then both will travel, the one to the *anode,* the other to the *cathode,* of the decomposing body.

iii. If, therefore, an *ion* pass towards one of the electrodes, another *ion* must also be passing simultaneously to the other electrode, although, from secondary action, it may not make its appearance.

iv. A body decomposable directly by the electric current, *i.e.* an *electrolyte,* must consist of two *ions,* and must also render them up during the act of decomposition.

v. There is but one *electrolyte* composed of the same two elementary *ions;* at least such appears to be the fact, dependent upon a law, that *only single electro-chemical equivalents of elementary ions can go to the electrodes, and not multiples.*

vi. A body not decomposable when alone, as boracic acid, is not directly decomposable by the electric current when in combination. It may act as an *ion* going wholly to the *anode* or *cathode,* but does not yield up its elements, except occasionally by a secondary action. Perhaps it is superfluous for me to point out that this proposition has *no relation* to such cases as that of water, which, by the presence of other bodies, is rendered a better conductor of electricity, and *therefore* is more freely decomposed.

vii. The nature of the substance of which the electrode is formed,

provided it be a conductor, causes no difference in the electro-decomposition, either in kind or degree: but it seriously influences, by secondary action, the state in which the *ions* finally appear. Advantage may be taken of this principle in combining and collecting such *ions* as, if evolved in their free state, would be unmanageable.

viii. A substance which, being used as the electrode, can combine with the *ion* evolved against it is also, I believe, an *ion,* and combines, in such cases, in the quantity represented by its *electro-chemical equivalent.* All the experiments I have made agree with this view; and it seems to me, at present, to result as a necessary consequence. Whether, in the secondary actions that take place, where the *ion* acts not upon the matter of the electrode but on that which is around it in the liquid, the same consequence follows, will require more extended investigation to determine.

ix. Compound *ions* are not necessarily composed of electro-chemical equivalents of simple *ions.* For instance, sulphuric acid, boracic acid, phosphoric acid are *ions,* but not *electrolytes, i.e.* not composed of electro-chemical equivalents of simple *ions.*

x. Electro-chemical equivalents are always consistent; *i.e.* the same number which represents the equivalent of a substance A when it is separating from a substance B will also represent A when separating from a third substance C. Thus, 8 is the electro-chemical equivalent of oxygen, whether separating from hydrogen, or tin, or lead; and 103.5 is the electro-chemical equivalent of lead, whether separating from oxygen, or chlorine, or iodine.

xi. Electro-chemical equivalents coincide, and are the same, with ordinary chemical equivalents.

By means of experiment and the preceding propositions, a knowledge of *ions* and their electro-chemical equivalents may be obtained in various ways.

In the first place, they may be determined directly, as has been done with hydrogen, oxygen, lead, and tin in the numerous experiments already quoted.

In the next place, from propositions ii and iii may be deduced the knowledge of many other *ions,* and also their equivalents. When chloride of lead was decomposed, platina being used for both electrodes, there could remain no more doubt that chlorine was passing to the *anode,* although it combined with the platina there, than when the positive electrode, being of plumbago, allowed its evolution in the free state; neither could there, in either case, remain any doubt that for every 103.5 parts of lead evolved at the *cathode,* 36 parts of chlorine were evolved at the *anode,* for the remaining chloride of lead was unchanged. So also, when in a metallic solution one volume of oxygen, or a secondary compound containing that proportion, appeared at the *anode,* no doubt could arise that hydrogen, equivalent to two volumes, had been determined to the *cathode,* although, by a secondary action, it had been employed in reducing oxides of lead, copper, or other metals to the metallic state. In this

manner, then, we learn from the experiments already described in these *Researches* that chlorine, iodine, bromine, fluorine, calcium, potassium, strontium, magnesium, manganese, etc., are *ions,* and that their *electro-chemical equivalents* are the same as their *ordinary chemical equivalents.*

Propositions iv and v extend our means of gaining information. For if a body of known chemical composition is found to be decomposable, and the nature of the substance evolved as a primary or even a secondary result at one of the electrodes be ascertained, the electro-chemical equivalent of that body may be deduced from the known constant composition of the substance evolved. Thus, when fused protiodide of tin is decomposed by the voltaic current the conclusion may be drawn that both the iodine and tin are *ions,* and that the proportions in which they combine in the fused compound express their electro-chemical equivalents. Again, with respect to the fused iodide of potassium, it is an electrolyte; and the chemical equivalents will also be the electro-chemical equivalents.

I think I cannot deceive myself in considering the doctrine of definite electro-chemical action as of the utmost importance. It touches by its facts, more directly and closely than any former fact, or set of facts, has done, upon the beautiful idea that ordinary chemical affinity is a mere consequence of the electrical attractions of the particles of different kinds of matter; and it will probably lead us to the means by which we may enlighten that which is at present so obscure, and either fully demonstrate the truth of the idea or develop that which ought to replace it.

On the absolute quantity of Electricity associated with the particles or atoms of Matter

The theory of definite electrolytical or electro-chemical action appears to me to touch immediately upon the *absolute quantity* of electricity or electric power belonging to different bodies. It is impossible, perhaps, to speak on this point without committing oneself beyond what present facts will sustain; and yet it is equally impossible, and perhaps would be impolitic, not to reason upon the subject. Although we know nothing of what an atom is, yet we cannot resist forming some idea of a small particle, which represents it to the mind; and though we are in equal, if not greater, ignorance of electricity, so as to be unable to say whether it is a particular matter or matters, or mere motion of ordinary matter, or some third kind of power or agent, yet there is an immensity of facts which justify us in believing that the atoms of matter are in some way endowed or associated with electrical powers, to which they owe their most striking qualities, and amongst them their mutual chemical affinity. As soon as we perceive, through the teaching of Dalton, that chemical powers are, however varied the circumstances in which they are exerted, definite for each body, we learn to estimate the relative degree of force which resides in such bodies; and when upon that knowledge comes the fact that the electricity, which we appear to be capable of loosening from its habitation

for a while and conveying from place to place, *whilst it retains its chemical force,* can be measured out, and being so measured is found to be *as definite in its action* as any of *those portions* which, remaining associated with the particles of matter, give them their *chemical relation;* we seem to have found the link which connects the proportion of that we have evolved to the proportion of that belonging to the particles in their natural state.

Now it is wonderful to observe how small a quantity of a compound body is decomposed by a certain portion of electricity. Let us, for instance, consider this and a few other points in relation to water. *One grain* of water, acidulated to facilitate conduction, will require an electric current to be continued for three minutes and three quarters of time to effect its decomposition, which current must be powerful enough to retain a platina wire $\frac{1}{104}$th of an inch in thickness, red hot, in the air during the whole time; and if interrupted anywhere by charcoal points, will produce a very brilliant and constant star of light. If attention be paid to the instantaneous discharge of electricity of tension, as illustrated in the beautiful experiments of Mr. Wheatstone, and to what I have said elsewhere on the relation of common and voltaic electricity, it will not be too much to say that this necessary quantity of electricity is equal to a very powerful flash of lightning. Yet we have it under perfect command; can evolve, direct, and employ it at pleasure; and when it has performed its full work of electrolysation, it has only separated the elements of *a single grain of water.*

On the other hand, the relation between the conduction of the electricity and the decomposition of the water is so close that one cannot take place without the other. If the water is altered only in that small degree which consists in its having the solid instead of the fluid state, the conduction is stopped, and the decomposition is stopped with it. Whether the conduction be considered as depending upon the decomposition or not, still the relation of the two functions is equally intimate and inseparable.

Considering this close and twofold relation, namely, that without decomposition transmission of electricity does not occur; and, that for a given definite quantity of electricity passed, an equally definite and constant quantity of water or other matter is decomposed; considering also that the agent, which is electricity, is simply employed in overcoming electrical powers in the body subjected to its action; it seems a probable and almost a natural consequence that the quantity which passes is the *equivalent* of, and therefore equal to, that of the particles separated; *i.e.* that if the electrical power which holds the elements of a grain of water in combination, or which makes a grain of oxygen and hydrogen in the right proportions unite into water when they are made to combine, could be thrown into the condition of *a current,* it would exactly equal the current required for the separation of that grain of water into its elements again.

This view of the subject gives an almost overwhelming idea of the

extraordinary quantity or degree of electric power which naturally belongs to the particles of matter; but it is not inconsistent in the slightest degree with the facts which can be brought to bear on this point. To illustrate this I must say a few words on the voltaic pile.

IV. ELECTRICITY OF THE VOLTAIC PILE

THOSE BODIES which, being interposed between the metals of the voltaic pile, render it active *are all of them electrolytes;* and it cannot but press upon the attention of everyone engaged in considering this subject that in those bodies (so essential to the pile) decomposition and the transmission of a current are so intimately connected that one cannot happen without the other. This I have shown abundantly in water, and numerous other cases. If, then, a voltaic trough have its extremities connected by a body capable of being decomposed, as water, we shall have a continuous current through the apparatus; and whilst it remains in this state we may look at the part where the acid is acting upon the plates and that where the current is acting upon the water as the reciprocals of each other. In both parts we have the two conditions *inseparable in such bodies as these,* namely, the passing of a current, and decomposition; and this is as true of the cells in the battery as of the water cell; for no voltaic battery has as yet been constructed in which the chemical action is only that of combination: *decomposition is always included,* and is, I believe, an essential chemical part.

But the difference in the two parts of the connected battery, that is, the decomposition or experimental cell and the acting cells, is simply this. In the former we urge the current through, but it, apparently of necessity, is accompanied by decomposition: in the latter we cause decompositions by ordinary chemical actions (which are, however, themselves electrical), and, as a consequence, have the electrical current; and as the decomposition dependent upon the current is definite in the former case, so is the current associated with the decomposition also definite in the latter.

Let us apply this in support of what I have surmised respecting the enormous electric power of each particle or atom of matter. I showed in a former part of these *Researches* on the relation by measure of common and voltaic electricity that two wires, one of platina and one of zinc, each one eighteenth of an inch in diameter, placed five sixteenths of an inch apart and immersed to the depth of five eighths of an inch in acid, consisting of one drop of oil of vitriol and four ounces of distilled water at a temperature of about 60° Fahr., and connected at the other extremities by a copper wire eighteen feet long and one eighteenth of an inch in thickness, yielded as much electricity in little more than three seconds of time as a Leyden battery charged by thirty turns of a very large and powerful plate electric machine in full action. This quantity, though sufficient if passed at once through the head of a rat or cat to have killed it, as by a flash of lightning, was evolved by the mutual action of so small a portion

of the zinc wire and water in contact with it that the loss of weight sustained by either would be inappreciable by our most delicate instruments; and as to the water which could be decomposed by that current, it must have been insensible in quantity, for no trace of hydrogen appeared upon the surface of the platina during those three seconds.

What an enormous quantity of electricity, therefore, is required for the decomposition of a single grain of water! We have already seen that it must be in quantity sufficient to sustain a platina wire $\frac{1}{104}$th of an inch in thickness, red hot, in contact with the air, for three minutes and three quarters, a quantity which is almost infinitely greater than that which could be evolved by the little standard voltaic arrangement to which I have just referred. I have endeavoured to make a comparison by the loss of weight of such a wire in a given time in such an acid, according to a principle and experiment to be almost immediately described; but the proportion is so high that I am almost afraid to mention it. It would appear that 800,000 such charges of the Leyden battery as I have referred to above would be necessary to supply electricity sufficient to decompose a single grain of water; or, if I am right, to equal the quantity of electricity which is naturally associated with the elements of that grain of water, endowing them with their mutual chemical affinity.

In further proof of this high electric condition of the particles of matter, and the *identity as to quantity of that belonging to them with that necessary for their separation,* I will describe an experiment of great simplicity but extreme beauty, when viewed in relation to the evolution of an electric current and its decomposing powers.

A dilute sulphuric acid, made by adding about one part by measure of oil of vitriol to thirty parts of water, will act energetically upon a piece of zinc plate in its ordinary and simple state; but, as Mr. Sturgeon has shown, not at all, or scarcely so, if the surface of the metal has in the first instance been amalgamated; yet the amalgamated zinc will act powerfully with platina as an electromotor, hydrogen being evolved on the surface of the latter metal, as the zinc is oxidised and dissolved. The amalgamation is best effected by sprinkling a few drops of mercury upon the surface of the zinc, the latter being moistened with the dilute acid, and rubbing with the fingers or tow so as to extend the liquid metal over the whole of the surface. Any mercury in excess, forming liquid drops upon the zinc, should be wiped off.

Two plates of zinc thus amalgamated were dried and accurately weighed; one, which we will call A, weighed 163.1 grains; the other, to be called B, weighed 148.3 grains. They were about five inches long and 0.4 of an inch wide. An earthenware pneumatic trough was filled with dilute sulphuric acid, of the strength just described, and a gas jar, also filled with the acid, inverted in it. A plate of platina of nearly the same length, but about three times as wide as the zinc plates, was put up into this jar. The zinc plate A was also introduced into the jar and brought in contact with the platina, and at the same moment the plate B was put into the acid of the trough, but out of contact with other metallic matter.

Strong action immediately occurred in the jar upon the contact of the zinc and platina plates. Hydrogen gas rose from the platina and was collected in the jar, but no hydrogen or other gas rose from *either* zinc plate. In about ten or twelve minutes, sufficient hydrogen having been collected, the experiment was stopped; during its progress a few small bubbles had appeared upon plate B, but none upon plate A. The plates were washed in distilled water, dried, and reweighed. Plate B weighed 148.3 grains, as before, having lost nothing by the direct chemical action of the acid. Plate A weighed 154.65 grains, 8.45 grains of it having been oxidised and dissolved during the experiment.

The hydrogen gas was next transferred to a water trough and measured; it amounted to 12.5 cubic inches, the temperature being 52° and the barometer 29.2 inches. This quantity, corrected for temperature, pressure, and moisture, becomes 12.15453 cubic inches of dry hydrogen at mean temperature and pressure; which, increased by one half for the oxygen that must have gone to the *anode, i.e.* to the zinc, gives 18.232 cubic inches as the quantity of oxygen and hydrogen evolved from the water decomposed by the electric current. According to the estimate of the weight of the mixed gas before adopted, this volume is equal to 2.3535544 grains, which therefore is the weight of water decomposed; and this quantity is to 8.45, the quantity of zinc oxidised, as 9 is to 32.31. Now taking 9 as the equivalent number of water, the number 32.5 is given as the equivalent number of zinc; a coincidence sufficiently near to show, what indeed could not but happen, that for an equivalent of zinc oxidised an equivalent of water must be decomposed.

But let us observe *how* the water is decomposed. It is electrolysed, *i.e.* is decomposed voltaically, and not in the ordinary manner (as to appearance) of chemical decompositions; for the oxygen appears at the *anode* and the hydrogen at the *cathode* of the body under decomposition, and these were in many parts of the experiment above an inch asunder. Again, the ordinary chemical affinity was not enough under the circumstances to effect the decomposition of the water, as was abundantly proved by the inaction on plate B; the voltaic current was essential. And to prevent any idea that the chemical affinity was almost sufficient to decompose the water, and that a smaller current of electricity might, under the circumstances, cause the hydrogen to pass to the *cathode,* I need only refer to the results which I have given to show that the chemical action at the electrodes has not the slightest influence over the *quantities* of water or other substances decomposed between them, but that they are entirely dependent upon the quantity of electricity which passes.

What, then, follows as a necessary consequence of the whole experiment? Why, this: that the chemical action upon 32.31 parts, or one equivalent of zinc, in this simple voltaic circle was able to evolve such quantity of electricity in the form of a current as, passing through water, should decompose 9 parts, or one equivalent of that substance: and considering the definite relations of electricity as developed in the preceding parts of the present paper, the results prove that the quantity of electricity

which, being naturally associated with the particles of matter, gives them their combining power is able, when thrown into a current, to separate those particles from their state of combination; or, in other words, that *the electricity which decomposes and that which is evolved by the decomposition of a certain quantity of matter are alike.*

The harmony which this theory of the definite evolution and the equivalent definite action of electricity introduces into the associated theories of definite proportions and electro-chemical affinity is very great. According to it, the equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity, or have naturally equal electric powers; it being the ELECTRICITY which *determines* the equivalent number, *because* it determines the combining force. Or, if we adopt the atomic theory or phraseology, then the atoms of bodies which are equivalents to each other in their ordinary chemical action have equal quantities of electricity naturally associated with them. But I must confess I am jealous of the term *atom;* for though it is very easy to talk of atoms, it is very difficult to form a clear idea of their nature, especially when compound bodies are under consideration.

But admitting that chemical action is the source of electricity, what an infinitely small fraction of that which is active do we obtain and employ in our voltaic batteries! Zinc and platina wires, one eighteenth of an inch in diameter and about half an inch long, dipped into dilute sulphuric acid so weak that it is not sensibly sour to the tongue, or scarcely to our most delicate test papers, will evolve more electricity in one twentieth of a minute than any man would willingly allow to pass through his body at once. The chemical action of a grain of water upon four grains of zinc can evolve electricity equal in quantity to that of a powerful thunderstorm. Nor is it merely true that the quantity is active; it can be directed and made to perform its full equivalent duty. Is there not, then, great reason to hope and believe that, by a closer *experimental* investigation of the principles which govern the development and action of this subtile agent, we shall be able to increase the power of our batteries or invent new instruments which shall a thousandfold surpass in energy those which we at present possess?

EXPERIMENTS IN PLANT-HYBRIDIZATION

by

GREGOR JOHANN MENDEL

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GREGOR JOHANN MENDEL

1822–1884

GREGOR JOHANN MENDEL channeled his energies into work in natural science and work in the church. His local contemporaries considered him a churchman who dabbled in scientific inquiry. Posterity has remembered him as a scientist who was, almost incidentally, a churchman. To Mendel, neither his scientific nor his church labors were interesting and important to the exclusion of the other, and he did not find his science and his religion incompatible. Indeed, the capabilities which made possible his scientific work were precisely those which brought him success as a churchman.

He was born on July 22, 1822, at Heinzendorf bei Odrau, in Austrian Silesia. His father, a small independent farmer, taught his son something of the art of grafting fruit trees, and later provided some means for the boy's formal education. His maternal uncle, Anton Schwirtlich, a man of intellectual tastes, had founded the first village school in Heinzendorf, and may well have encouraged his nephew's ambitions for schooling. His younger sister contributed to the cost of his education a portion of her dowry—a loan he subsequently more than repaid by providing for the education of her three sons. Concerning other members of the family, there is practically no pertinent information.

Young Mendel attended the local school founded by his uncle, then the government school which succeeded it, then the better school at neighboring Leipnik. There he so distinguished himself as a student that his family strained its financial means to afford him the gymnasium at Troppau and a subsequent year at Olmütz. During his residence at Troppau an Augustinian monk, one of his teachers, apparently showed the young man a way to a career in education—membership in a religious order. In consequence, Mendel applied for membership in the Augustinian House of St. Thomas in Brünn

(the Königskloster), was accepted, took the name Gregor "in religion"—he had been baptized Johann—and devoted himself to the institution's program in education. In 1847 he was ordained a priest; in 1851, at the expense of the cloister, he went to Vienna for two years of study in the natural sciences; from 1853 to 1868 he taught scientific subjects, especially physics, in the Realschule at Brünn. In the latter year he was elected Abbot of the Königskloster.

As the Abbot of the Cloister, Mendel became involved in a dispute with the government which lasted during his whole tenure of office, until his death in 1884. There had been enacted in 1872 a measure imposing special taxes on the property of religious houses. Mendel held the exaction to be unjust, to be special legislation which distinguished inequitably among various property holders; he refused payment of the sum levied against the Königskloster. Several other monasteries similarly refused payment, for the same reason. These others gradually retreated from their position, cajoled and threatened and persuaded by government agents who offered compromises and concessions. Finally, only the Königskloster stood by its abbot's original declaration of principles. Mendel lost friends; the property of the Cloister was distrained upon; lawsuits multiplied. Still the abbot stood firm. Even at the time of his death the question remained unsolved. But a few years later the tax was quietly removed.

The same stiff-necked adherence to position, the same pertinacity, the same thorough attention to detail, which enabled Mendel for so many years to oppose the government, distinguish his scientific work. This work began about the time he entered the Augustinian Order and continued for a full twenty-two years—until his election as Abbot. Thereafter the administrative responsibility he had to carry prevented further experimentation, and his later years are, for the historian of science, wholly barren.

During the twenty-two years of his active experimenting, Mendel interested himself in a variety of subjects. He studied sunspots, kept a close record of meteorological phenomena and published his observations annually in the Brünn *Abhandlüngen,* established fifty beehives in the Cloister gardens in order to study heredity, and so on. What he learned from his bees is now not known, for his notes have been lost—possibly destroyed by Mendel himself. His interest in heredity led him, however, also to the study of peas, and out of that study he developed the theory which under the name Mendelism has won more adherents than any other in the last fifty years.

At least as early as 1855 the problems of heredity interested Mendel. At that time half a dozen naturalists and

hybridizers were attempting to solve the problem of the origin of species; Darwin's ideas, widely circulated in his magnificent books, had not yet persuaded the whole scientific world. Mendel stated the problem to himself with great clarity, chose as his material the edible pea because the varieties in cultivation are distinguished from one another by striking characteristics easy to recognize, and then settled down to eight years of planting, cross-fertilizing, reaping, and replanting. Above all, he undertook the wearisome, endless labor of constantly recording results. From these results he proceeded to the penetrating analysis which permitted him, in 1865, to read a report of his findings to the Academy of Brünn. The following year the Academy published his report.

Familiar with the work of earlier hybridizers, Mendel saw that for success he would need, unlike his predecessors, to consider separately each characteristic of the strain he was breeding; that he would need to keep each generation quite distinct from each other generation; that he would need to record separately the progeny from different individuals. When he had decided to use edible peas as his subject, he chose a pair of varieties of which one was tall—six to seven feet—and one dwarf—nine to eighteen inches. He then cross-bred these varieties to discover to what height the progeny would grow. He called *tallness* one characteristic and named it *dominant;* he called *dwarfness* an opposite characteristic and named it *recessive.*

In the wide Cloister gardens Mendel raised generation after generation of his peas, always planting all the seed obtained from the last preceding experiment. He studied the contrasting characteristics of *smoothness* and *rugosity,* of *green* and *yellow* coloration in the cotyledons, and so on. Eventually his plantings occupied a great part of the garden space. Similarly, his results occupy a great space in the theories of genetics.

When he had completed his work with peas, Mendel did a series of similar experiments with Hieracium, and gave the results, which corroborated those of the experiments with peas, to the Brünn Academy in 1869. Then his work as abbot began, and he did no further experimenting.

The publication of Mendel's two papers in the *Proceedings* of the Brünn Academy attracted literally no attention. Darwin had printed the *Origin* in 1859, so startling the scientific and lay worlds, so amazing and almost stupefying them, that no one paid heed to the records of the quiet, unknown Augustinian. He did not himself, apparently, send copies of his papers to the great naturalists whose work he knew and revered. Though the Academy of Brünn exchanged publica-

tions with other European academies, including the Royal and Linnaean societies of London, no one read Mendel's papers with any appreciation of the work he had done or of the importance attaching to his conclusions.

Meantime the pertinacity, the devotion, the tireless skill which Mendel had given to his biological experiments he was now dedicating to the administrative labors of the Königskloster. The quarrel with the government heightened. His health began to fail. In 1884 he died—still unknown among the scientists.

Sixteen years later, in 1900, within a few months, three naturalists—de Vries, Correns, and Tschermak—published independent papers each giving the substance of Mendel's treatise and each confirming it. A year later the original paper appeared in an English translation in the *Journal* of the Royal Horticultural Society. In 1902, W. Bateson published a revised translation, the major part of which follows. Mendel's time of fame had come, and it endures.

EXPERIMENTS IN PLANT-HYBRIDIZATION

INTRODUCTORY REMARKS

EXPERIENCE of artificial fertilization, such as is effected with ornamental plants in order to obtain new variations in colour, has led to the experiments which will here be discussed. The striking regularity with which the same hybrid forms always reappeared whenever fertilization took place between the same species induced further experiments to be undertaken, the object of which was to follow up the developments of the hybrids in their progeny.

Those who survey the work done in this department will arrive at the conviction that among all the numerous experiments made, not one has been carried out to such an extent and in such a way as to make it possible to determine the number of different forms under which the offspring of hybrids appear, or to arrange these forms with certainty according to their separate generations, or definitely to ascertain their statistical relations.

It requires indeed some courage to undertake a labour of such far-reaching extent; this appears, however, to be the only right way by which we can finally reach the solution of a question the importance of which cannot be overestimated in connection with the history of the evolution of organic forms.

The paper now presented records the results of such a detailed experiment. This experiment was practically confined to a small plant group, and is now, after eight years' pursuit, concluded in all essentials. Whether the plan upon which the separate experiments were conducted and carried out was the best suited to attain the desired end is left to the friendly decision of the reader.

SELECTION OF THE EXPERIMENTAL PLANTS

THE VALUE and utility of any experiment are determined by the fitness of the material to the purpose for which it is used, and thus in the case before us it cannot be immaterial what plants are subjected to experiment and in what manner such experiments are conducted.

The selection of the plant group which shall serve for experiments of

this kind must be made with all possible care if it be desired to avoid from the outset every risk of questionable results.

The experimental plants must necessarily—

1. Possess constant differentiating characters.

2. The hybrids of such plants must, during the flowering period, be protected from the influence of all foreign pollen, or be easily capable of such protection.

The hybrids and their offspring should suffer no marked disturbance in their fertility in the successive generations.

Accidental impregnation by foreign pollen, if it occurred during the experiments and were not recognized, would lead to entirely erroneous conclusions. Reduced fertility or entire sterility of certain forms, such as occurs in the offspring of many hybrids, would render the experiments very difficult or entirely frustrate them. In order to discover the relations in which the hybrid forms stand towards each other and also towards their progenitors it appears to be necessary that all members of the series developed in each successive generation should be, *without exception,* subjected to observation.

At the very outset special attention was devoted to the *Leguminosae* on account of their peculiar floral structure. Experiments which were made with several members of this family led to the result that the genus *Pisum* was found to possess the necessary qualifications.

Some thoroughly distinct forms of this genus possess characters which are constant, and easily and certainly recognizable, and when their hybrids are mutually crossed they yield perfectly fertile progeny. Furthermore, a disturbance through foreign pollen cannot easily occur, since the fertilizing organs are closely packed inside the keel and the anther bursts within the bud, so that the stigma becomes covered with pollen even before the flower opens. This circumstance is of especial importance. As additional advantages worth mentioning, there may be cited the easy culture of these plants in the open ground and in pots, and also their relatively short period of growth. Artificial fertilization is certainly a somewhat elaborate process, but nearly always succeeds. For this purpose the bud is opened before it is perfectly developed, the keel is removed, and each stamen carefully extracted by means of forceps, after which the stigma can at once be dusted over with the foreign pollen.

DIVISION AND ARRANGEMENT OF THE EXPERIMENTS

If two plants which differ constantly in one or several characters be crossed, numerous experiments have demonstrated that the common characters are transmitted unchanged to the hybrids and their progeny; but each pair of differentiating characters, on the other hand, unite in the hybrid to form a new character, which in the progeny of the hybrid is usually variable. The object of the experiment was to observe these variations in the case of each pair of differentiating characters, and to deduce

the law according to which they appear in the successive generations. The experiment resolves itself therefore into just as many separate experiments as there are constantly differentiating characters presented in the experimental plants.

The various forms of Peas selected for crossing showed differences in the length and colour of the stem; in the size and form of the leaves; in the position, colour, and size of the flowers; in the length of the flower stalk; in the colour, form, and size of the pods; in the form and size of the seeds; and in the colour of the seed coats and of the albumen [cotyledons]. Some of the characters noted do not permit of a sharp and certain separation, since the difference is of a "more or less" nature, which is often difficult to define. Such characters could not be utilized for the separate experiments; these could only be applied to characters which stand out clearly and definitely in the plants. Lastly, the result must show whether they, in their entirety, observe a regular behaviour in their hybrid unions, and whether from these facts any conclusion can be come to regarding those characters which possess a subordinate significance in the type.

The characters which were selected for experiment relate:

1. To the *difference in the form of the ripe seeds.* These are either round or roundish, the depressions, if any, occur on the surface, being always only shallow; or they are irregularly angular and deeply wrinkled (*P. quadratum*).

2. To the *difference in the colour of the seed albumen* (endosperm). The albumen of the ripe seeds is either pale yellow, bright yellow and orange coloured, or it possesses a more or less intense green tint. This difference of colour is easily seen in the seeds as their coats are transparent.

3. To the *difference in the colour of the seed coat.* This is either white, with which character white flowers are constantly correlated; or it is grey, grey-brown, leather-brown, with or without violet spotting, in which case the colour of the standards is violet, that of the wings purple, and the stem in the axils of the leaves is of a reddish tint. The grey seed coats become dark brown in boiling water.

4. To the *difference in the form of the ripe pods.* These are either simply inflated, not contracted in places; or they are deeply constricted between the seeds and more or less wrinkled (*P. saccharatum*).

5. To the *difference in the colour of the unripe pods.* They are either light to dark green, or vividly yellow, in which colouring the stalks, leaf veins, and calyx participate.

6. To the *difference in the position of the flowers.* They are either axial, that is, distributed along the main stem; or they are terminal, that is, bunched at the top of the stem and arranged almost in a false umbel; in this case the upper part of the stem is more or less widened in section (*P. umbellatum*).

7. To the *difference in the length of the stem.* The length of the stem is very various in some forms; it is, however, a constant character for each,

in so far that healthy plants, grown in the same soil, are only subject to unimportant variations in this character.

In experiments with this character, in order to be able to discriminate with certainty, the long axis of 6 to 7 ft. was always crossed with the short one of ¾ ft. to 1½ ft.

Each two of the differentiating characters enumerated above were united by cross-fertilization. There were made for the

1st	trial	60	fertilizations	on	15	plants.
2nd	"	58	"	"	10	"
3rd	"	35	"	"	10	"
4th	"	40	"	"	10	"
5th	"	23	"	"	5	"
6th	"	34	"	"	10	"
7th	"	37	"	"	10	"

Furthermore, in all the experiments reciprocal crossings were effected in such a way that each of the two varieties which in one set of fertilizations served as seed bearer in the other set was used as the pollen plant.

The plants were grown in garden beds, a few also in pots, and were maintained in their naturally upright position by means of sticks, branches of trees, and strings stretched between. For each experiment a number of pot plants were placed during the blooming period in a greenhouse, to serve as control plants for the main experiment in the open as regards possible disturbance by insects. Among the insects which visit Peas the beetle *Bruchus pisi* might be detrimental to the experiments should it appear in numbers. The female of this species is known to lay the eggs in the flower, and in so doing opens the keel; upon the tarsi of one specimen, which was caught in a flower, some pollen grains could clearly be seen under a lens.

The risk of false impregnation by foreign pollen is, however, a very slight one with *Pisum,* and is quite incapable of disturbing the general result. Among more than 10,000 plants which were carefully examined there were only a very few cases where an indubitable false impregnation had occurred. Since in the greenhouse such a case was never remarked, it may well be supposed that *Bruchus pisi,* and possibly also abnormalities in the floral structure, were to blame.

[F_1] *THE FORMS OF THE HYBRIDS*

Experiments which in previous years were made with ornamental plants have already afforded evidence that the hybrids, as a rule, are not exactly intermediate between the parental species. With some of the more striking characters, those, for instance, which relate to the form and size of the leaves, the pubescence of the several parts, &c., the intermediate, indeed, is nearly always to be seen; in other cases, however, one of the two parental characters is so preponderant that it is difficult, or quite impossible, to detect the other in the hybrid.

This is precisely the case with the Pea hybrids. In the case of each of the seven crosses the hybrid character resembles that of one of the parental forms so closely that the other either escapes observation completely or cannot be detected with certainty. This circumstance is of great importance in the determination and classification of the forms under which the offspring of the hybrids appear. Henceforth in this paper those characters which are transmitted entire, or almost unchanged in the hybridization, and therefore in themselves constitute the characters of the hybrid, are termed the *dominant,* and those which become latent in the process *recessive.* The expression "recessive" has been chosen because the characters thereby designated withdraw or entirely disappear in the hybrids, but nevertheless reappear unchanged in their progeny, as will be demonstrated later on.

It was furthermore shown by the whole of the experiments that it is perfectly immaterial whether the dominant characters belong to the seed bearer or to the pollen parent; the form of the hybrid remains identical in both cases.

Of the differentiating characters which were used in the experiments the following are dominant:

1. The round or roundish form of the seed with or without shallow depressions.
2. The yellow colouring of the seed albumen [cotyledons].
3. The grey, grey-brown, or leather-brown colour of the seed coat, in association with violet-red blossoms and reddish spots in the leaf axils.
4. The simply inflated form of the pod.
5. The green colouring of the unripe pod in association with the same colour in the stems, the leaf veins, and the calyx.
6. The distribution of the flowers along the stem.
7. The greater length of stem.

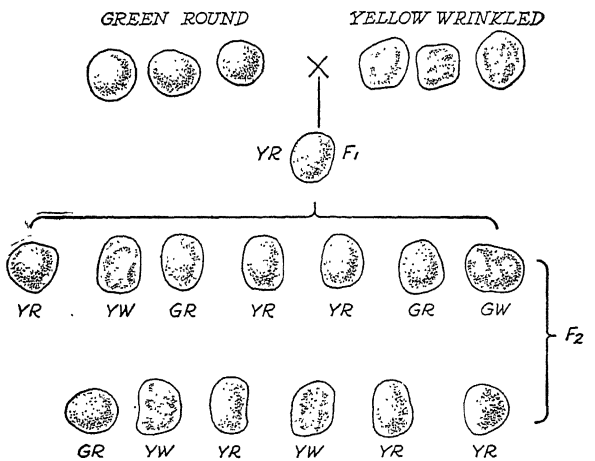
With regard to this last character it must be stated that the longer of the two parental stems is usually exceeded by the hybrid, a fact which is possibly only attributable to the greater luxuriance which appears in all parts of plants when stems of very different length are crossed. Thus, for instance, in repeated experiments, stems of 1 ft. and 6 ft. in length yielded without exception hybrids which varied in length between 6 ft. and 7½ ft.

[F_2] *THE FIRST GENERATION [BRED] FROM THE HYBRIDS*

IN THIS GENERATION there reappear, together with the dominant characters, also the recessive ones with their peculiarities fully developed, and this occurs in the definitely expressed average proportion of three to one, so that among each four plants of this generation three display the dominant character and one the recessive. This relates without exception to all the characters which were investigated in the experiments. The angular wrinkled form of the seed, the green colour of the albumen, the white

colour of the seed coats and the flowers, the constrictions of the pods, the yellow colour of the unripe pod, of the stalk, of the calyx, and of the leaf venation, the umbel-like form of the inflorescence, and the dwarfed stem, all reappear in the numerical proportion given, without any essential alteration. *Transitional forms were not observed in any experiment.*

Since the hybrids resulting from reciprocal crosses are formed alike and present no appreciable difference in their subsequent development, consequently the results [of the reciprocal crosses] can be reckoned together in each experiment. The relative numbers which were obtained for each pair of differentiating characters are as follows:



INHERITANCE OF SEED CHARACTERS IN PEA

The seed of a green round variety fertilized by pollen of a yellow wrinkled variety are yellow and round (F_1). The reciprocal cross would give the same result. Two pods of F_2 seed borne by the F_1 plant are shown. There were 6 yellow round, 3 green round, 3 yellow wrinkled, 1 green wrinkled.

Expt. 1. Form of seed.—From 253 hybrids 7,324 seeds were obtained in the second trial year. Among them were 5,474 round or roundish ones and 1,850 angular wrinkled ones. Therefrom the ratio 2.96 to 1 is deduced.

Expt. 2. Colour of albumen.—258 plants yielded 8,023 seeds, 6,022 yellow, and 2,001 green; their ratio, therefore, is as 3.01 to 1.

In these two experiments each pod yielded usually both kinds of seed. In well-developed pods which contained on the average six to nine seeds, it often happened that all the seeds were round (Expt. 1) or all yellow (Expt. 2); on the other hand there were never observed more than five wrinkled or five green ones in one pod. It appears to make no difference whether the pods are developed early or later in the hybrid or whether they spring from the main axis or from a lateral one. In some few plants

only a few seeds developed in the first formed pods, and these possessed exclusively one of the two characters, but in the subsequently developed pods the normal proportions were maintained nevertheless.

These two experiments are important for the determination of the average ratios, because with a smaller number of experimental plants they show that very considerable fluctuations may occur. In counting the seeds, also, especially in Expt. 2, some care is requisite, since in some of the seeds of many plants the green colour of the albumen is less developed, and at first may be easily overlooked. The cause of this partial disappearance of the green colouring has no connection with the hybrid character of the plants, as it likewise occurs in the parental variety. This peculiarity [bleaching] is also confined to the individual and is not inherited by the offspring. In luxuriant plants this appearance was frequently noted. Seeds which are damaged by insects during their development often vary in colour and form, but, with a little practice in sorting, errors are easily avoided. It is almost superfluous to mention that the pods must remain on the plants until they are thoroughly ripened and have become dried, since it is only then that the shape and colour of the seed are fully developed.

Expt. 3. Colour of the seed coats.—Among 929 plants 705 bore violet-red flowers and grey-brown seed coats; 224 had white flowers and white seed coats, giving the proportion 3.15 to 1.

Expt. 4. Form of pods.—Of 1,181 plants 882 had them simply inflated, and in 299 they were constricted. Resulting ratio, 2.95 to 1.

Expt. 5. Colour of the unripe pods.—The number of trial plants was 580, of which 428 had green pods and 152 yellow ones. Consequently these stand in the ratio 2.82 to 1.

Expt. 6. Position of flowers.—Among 858 cases 651 had inflorescences axial and 207 terminal. Ratio, 3.14 to 1.

Expt. 7. Length of stem.—Out of 1,064 plants, in 787 cases the stem was long, and in 277 short. Hence a mutual ratio of 2.84 to 1. In this experiment the dwarfed plants were carefully lifted and transferred to a special bed. This precaution was necessary, as otherwise they would have perished through being overgrown by their tall relatives. Even in their quite young state they can be easily picked out by their compact growth and thick dark-green foliage.

If now the results of the whole of the experiments be brought together, there is found, as between the number of forms with the dominant and recessive characters, an average ratio of 2.98 to 1, or 3 to 1.

The dominant character can have here a *double signification*—viz. that of a parental character, or a hybrid character. In which of the two significations it appears in each separate case can only be determined by the following generation. As a parental character it must pass over unchanged to the whole of the offspring; as a hybrid character, on the other hand, it must maintain the same behaviour as in the first generation [F_2].

[F_3] *THE SECOND GENERATION [BRED] FROM THE HYBRIDS*

THOSE FORMS which in the first generation [F_2] exhibit the recessive character do not further vary in the second generation [F_3] as regards this character; they remain constant in their offspring.

It is otherwise with those which possess the dominant character in the first generation [bred from the hybrids]. Of these *two* thirds yield offspring which display the dominant and recessive characters in the proportion of 3 to 1, and thereby show exactly the same ratio as the hybrid forms, while only *one* third remains with the dominant character constant.

The separate experiments yielded the following results:

Expt. 1. Among 565 plants which were raised from round seeds of the first generation, 193 yielded round seeds only, and remained therefore constant in this character; 372, however, gave both round and wrinkled seeds, in the proportion of 3 to 1. The number of the hybrids, therefore, as compared with the constants is 1.93 to 1.

Expt. 2. Of 519 plants which were raised from seeds whose albumen was of yellow colour in the first generation, 166 yielded exclusively yellow, while 353 yielded yellow and green seeds in the proportion of 3 to 1. There resulted, therefore, a division into hybrid and constant forms in the proportion of 2.13 to 1.

For each separate trial in the following experiments 100 plants were selected which displayed the dominant character in the first generation, and in order to ascertain the significance of this, ten seeds of each were cultivated.

Expt. 3. The offspring of 36 plants yielded exclusively grey-brown seed coats, while of the offspring of 64 plants some had grey-brown and some had white.

Expt. 4. The offspring of 29 plants had only simply inflated pods; of the offspring of 71, on the other hand, some had inflated and some constricted.

Expt. 5. The offspring of 40 plants had only green pods; of the offspring of 60 plants some had green, some yellow ones.

Expt. 6. The offspring of 33 plants had only axial flowers; of the offspring of 67, on the other hand, some had axial and some terminal flowers.

Expt. 7. The offspring of 28 plants inherited the long axis, and those of 72 plants some the long and some the short axis.

In each of these experiments a certain number of the plants came constant with the dominant character. For the determination of the proportion in which the separation of the forms with the constantly persistent character results, the two first experiments are of especial importance, since in these a larger number of plants can be compared. The

ratios 1.93 to 1 and 2.13 to 1 gave together almost exactly the average ratio of 2 to 1. The sixth experiment gave a quite concordant result; in the others the ratio varies more or less, as was only to be expected in view of the smaller number of 100 trial plants. Experiment 5, which shows the greatest departure, was repeated, and then, in lieu of the ratio of 60 and 40, that of 65 and 35 resulted. *The average ratio of 2 to 1 appears, therefore, as fixed with certainty.* It is therefore demonstrated that, of those forms which possess the dominant character in the first generation, two thirds have the hybrid character, while one third remains constant with the dominant character.

The ratio of 3 to 1, in accordance with which the distribution of the dominant and recessive characters results in the first generation, resolves itself therefore in all experiments into the ratio of 2 : 1 : 1 if the dominant character be differentiated according to its significance as a hybrid character or as a parental one. Since the members of the first generation [F_2] spring directly from the seed of the hybrids [F_1], *it is now clear that the hybrids form seeds having one or other of the two differentiating characters, and of these one half develop again the hybrid form, while the other half yield plants which remain constant and receive the dominant or the recessive characters [respectively] in equal numbers.*

THE SUBSEQUENT GENERATIONS [BRED] FROM THE HYBRIDS

THE PROPORTIONS in which the descendants of the hybrids develop and split up in the first and second generations presumably hold good for all subsequent progeny. Experiments 1 and 2 have already been carried through six generations, 3 and 7 through five, and 4, 5, and 6 through four, these experiments being continued from the third generation with a small number of plants, and no departure from the rule has been perceptible. The offspring of the hybrids separated in each generation in the ratio of 2 : 1 : 1 into hybrids and constant forms.

If A be taken as denoting one of the two constant characters, for instance the dominant, a, the recessive, and Aa the hybrid form in which both are conjoined, the expression

$$A+2Aa+a$$

shows the terms in the series for the progeny of the hybrids of two differentiating characters.

The observation made by Gärtner, Kölreuter, and others, that hybrids are inclined to revert to the parental forms, is also confirmed by the experiments described. It is seen that the number of the hybrids which arise from one fertilization, as compared with the number of forms which become constant, and their progeny from generation to generation, is continually diminishing, but that nevertheless they could not entirely disappear. If an average equality of fertility in all plants in all generations be

assumed, and if, furthermore, each hybrid forms seed of which one half yields hybrids again, while the other half is constant to both characters in equal proportions, the ratio of numbers for the offspring in each generation is seen by the following summary, in which *A* and *a* denote again the two parental characters, and *Aa* the hybrid forms. For brevity's sake it may be assumed that each plant in each generation furnishes only 4 seeds.

Generation	*A*	*Aa*	*a*	Ratios *A* : *Aa* : *a*
1	1	2	1	1 : 2 : 1
2	6	4	6	3 : 2 : 3
3	28	8	28	7 : 2 : 7
4	120	16	120	15 : 2 : 15
5	496	32	496	31 : 2 : 31
n				2^n-1 : 2 : 2^n-1

In the tenth generation, for instance, $2^n-1=1023$. There result, therefore, in each 2,048 plants which arise in this generation, 1,023 with the constant dominant character, 1,023 with the recessive character, and only two hybrids.

THE OFFSPRING OF HYBRIDS IN WHICH SEVERAL DIFFERENTIATING CHARACTERS ARE ASSOCIATED

In the experiments above described plants were used which differed only in one essential character. The next task consisted in ascertaining whether the law of development discovered in these applied to each pair of differentiating characters when several diverse characters are united in the hybrid by crossing. As regards the form of the hybrids in these cases, the experiments showed throughout that this invariably more nearly approaches to that one of the two parental plants which possesses the greater number of dominant characters. If, for instance, the seed plant has a short stem, terminal white flowers, and simply inflated pods; the pollen plant, on the other hand, a long stem, violet-red flowers distributed along the stem, and constricted pods; the hybrid resembles the seed parent only in the form of the pod; in the other characters it agrees with the pollen parent. Should one of the two parental types possess only dominant characters, then the hybrid is scarcely or not at all distinguishable from it.

Two experiments were made with a considerable number of plants. In the first experiment the parental plants differed in the form of the seed and in the colour of the albumen; in the second in the form of the seed, in the colour of the albumen, and in the colour of the seed coats. Experiments with seed characters give the result in the simplest and most certain way.

In order to facilitate study of the data in these experiments, the different characters of the seed plant will be indicated by *A, B, C,* those of the pollen plant by *a, b, c,* and the hybrid forms of the characters by *Aa, Bb,* and *Cc.*

Expt. 1.—*AB*, seed parents; *ab*, pollen parents;
A, form round; *a*, form wrinkled;
B, albumen yellow. *b*, albumen green.

The fertilized seeds appeared round and yellow like those of the seed parents. The plants raised therefrom yielded seeds of four sorts, which frequently presented themselves in one pod. In all, 556 seeds were yielded by 15 plants, and of these there were:

315 round and yellow,
101 wrinkled and yellow,
108 round and green,
32 wrinkled and green.

All were sown the following year. Eleven of the round yellow seeds did not yield plants, and three plants did not form seeds. Among the rest:

38 had round yellow seeds ..... *AB*
65 round yellow and green seeds ..... *ABb*
60 round yellow and wrinkled yellow seeds ..... *AaB*
138 round yellow and green, wrinkled yellow and green seeds ..... *AaBb*.

From the wrinkled yellow seeds 96 resulting plants bore seed, of which:

28 had only wrinkled yellow seeds ..... *aB*
68 wrinkled yellow and green seeds ..... *aBb*.

From 108 round green seeds 102 resulting plants fruited, of which:

35 had only round green seeds ..... *Ab*
67 round and wrinkled green seeds ..... *Aab*.

The wrinkled green seeds yielded 30 plants which bore seeds all of like character; they remained constant *ab*.

The offspring of the hybrids appeared therefore under nine different forms, some of them in very unequal numbers. When these are collected and co-ordinated we find:

38 plants with the sign *AB*
35 " " " *Ab*
28 " " " *aB*
30 " " " *ab*
65 " " " *ABb*
68 " " " *aBb*
60 " " " *AaB*
67 " " " *Aab*
138 " " " *AaBb*.

The whole of the forms may be classed into three essentially different groups. The first includes those with the signs *AB*, *Ab*, *aB*, and *ab*: they

possess only constant characters and do not vary again in the next generation. Each of these forms is represented on the average thirty-three times. The second group includes the signs *ABb, aBb, AaB, Aab:* these are constant in one character and hybrid in another, and vary in the next generation only as regards the hybrid character. Each of these appears on an average sixty-five times. The form *AaBb* occurs 138 times: it is hybrid in both characters, and behaves exactly as do the hybrids from which it is derived.

If the numbers in which the forms belonging to these classes appear be compared, the ratios of 1, 2, 4 are unmistakably evident. The numbers 32, 65, 138 present very fair approximations to the ratio numbers of 33, 66, 132.

The developmental series consists, therefore, of nine classes, of which four appear therein always once and are constant in both characters; the forms *AB, ab,* resemble the parental forms, the two others present combinations between the conjoined characters *A, a, B, b,* which combinations are likewise possibly constant. Four classes appear always twice, and are constant in one character and hybrid in the other. One class appears four times, and is hybrid in both characters. Consequently the offspring of the hybrids, if two kinds of differentiating characters are combined therein, are represented by the expression

$$AB+Ab+aB+ab+2ABb+2aBb+2AaB+2Aab+4AaBb.$$

This expression is indisputably a combination series in which the two expressions for the characters *A* and *a, B* and *b* are combined. We arrive at the full number of the classes of the series by the combination of the expressions:

$$A+2Aa+a$$
$$B+2Bb+b.$$

Expt. 2.

ABC, seed parents;
A, form round;
B, albumen yellow;
C, seed coat grey-brown.

abc, pollen parents;
a, form wrinkled;
b, albumen green;
c, seed coat white.

This experiment was made in precisely the same way as the previous one. Among all the experiments it demanded the most time and trouble. From 24 hybrids 687 seeds were obtained in all: these were all either spotted, grey-brown or grey-green, round or wrinkled. From these in the following year 639 plants fruited, and, as further investigation showed, there were among them:

8	plants	*ABC*	22	plants	*ABCc*	45	plants	*ABbCc*
14	"	*ABc*	17	"	*AbCc*	36	"	*aBbCc*
9	"	*AbC*	25	"	*aBCc*	38	"	*AaBCc*
11	"	*Abc*	20	"	*abCc*	40	"	*AaBCc*
8	"	*aBC*	15	"	*ABbC*	49	"	*AaBbC*

10	plants *aBc*	18	plants *ABbc*	48	plants *AaBbc*
10	" *abC*	19	" *aBbC*		
7	" *abc*	24	" *aBbc*		
		14	" *AaBC*	78	" *AaBbCc*
		18	" *AaBc*		
		20	" *AabC*		
		16	" *Aabc*		

The whole expression contains 27 terms. Of these 8 are constant in all characters, and each appears on the average 10 times; 12 are constant in two characters and hybrid in the third; each appears on the average 19 times; 6 are constant in one character and hybrid in the other two; each appears on the average 43 times. One form appears 78 times and is hybrid in all of the characters. The ratios 10, 19, 43, 78 agree so closely with the ratios 10, 20, 40, 80, or 1, 2, 4, 8, that this last undoubtedly represents the true value.

The development of the hybrids when the original parents differ in three characters results therefore according to the following expression:

$$ABC + ABc + AbC + Abc + aBC + aBc + abC + abc + 2ABCc + 2AbCc + 2aBCc + 2abCc + 2ABbC + 2ABbc + 2aBbC + 2aBbc + 2AaBC + 2AaBc + 2AabC + 2Aabc + 4ABbCc + 4aBbCc + 4AaBCc + 4AabCc + 4AaBbC + 4AaBbc + 8AaBbCc.$$

Here also is involved a combination series in which the expressions for the characters *A* and *a*, *B* and *b*, *C* and *c*, are united. The expressions

$$A+2Aa+a$$
$$B+2Bb+b$$
$$C+2Cc+c$$

give all the classes of the series. The constant combinations which occur therein agree with all combinations which are possible between the characters *A, B, C, a, b, c;* two thereof, *ABC* and *abc,* resemble the two original parental stocks.

In addition, further experiments were made with a smaller number of experimental plants in which the remaining characters by twos and threes were united as hybrids: all yielded approximately the same results. There is therefore no doubt that for the whole of the characters involved in the experiments the principle applies that *the offspring of the hybrids in which several essentially different characters are combined exhibit the terms of a series of combinations, in which the developmental series for each pair of differentiating characters are united.* It is demonstrated at the same time that *the relation of each pair of different characters in hybrid union is independent of the other differences in the two original parental stocks.*

If n represent the number of the differentiating characters in the two original stocks, 3^n gives the number of terms of the combination series, 4^n the number of individuals which belong to the series, and 2^n the number of unions which remain constant. The series therefore contains, if the

original stocks differ in four characters, $3^4=81$ classes, $4^4=256$ individuals, and $2^4=16$ constant forms; or, which is the same, among each 256 offspring of the hybrids there are 81 different combinations, 16 of which are constant.

All constant combinations which in Peas are possible by the combination of the said seven differentiating characters were actually obtained by repeated crossing. Their number is given by $2^7=128$. Thereby is simultaneously given the practical proof *that the constant characters which appear in the several varieties of a group of plants may be obtained in all the associations which are possible according to the* [*mathematical*] *laws of combination, by means of repeated artificial fertilization.*

If we endeavour to collate in a brief form the results arrived at, we find that those differentiating characters, which admit of easy and certain recognition in the experimental plants, all behave exactly alike in their hybrid associations. The offspring of the hybrids of each pair of differentiating characters are, one half, hybrid again, while the other half are constant in equal proportions having the characters of the seed and pollen parents respectively. If several differentiating characters are combined by cross-fertilization in a hybrid, the resulting offspring form the terms of a combination series in which the combination series for each pair of differentiating characters are united.

The uniformity of behaviour shown by the whole of the characters submitted to experiment permits, and fully justifies, the acceptance of the principle that a similar relation exists in the other characters which appear less sharply defined in plants, and therefore could not be included in the separate experiments. An experiment with peduncles of different lengths gave on the whole a fairly satisfactory result, although the differentiation and serial arrangement of the forms could not be effected with that certainty which is indispensable for correct experiment.

THE REPRODUCTIVE CELLS OF THE HYBRIDS

THE RESULTS of the previously described experiments led to further experiments, the results of which appear fitted to afford some conclusions as regards the composition of the egg and pollen cells of hybrids. An important clue is afforded in *Pisum* by the circumstance that among the progeny of the hybrids constant forms appear, and that this occurs, too, in respect of all combinations of the associated characters. So far as experience goes, we find it in every case confirmed that constant progeny can only be formed when the egg cells and the fertilizing pollen are of like character, so that both are provided with the material for creating quite similar individuals, as is the case with the normal fertilization of pure species. We must therefore regard it as certain that exactly similar factors must be at work also in the production of the constant forms in the hybrid plants. Since the various constant forms are produced in *one* plant, or even in *one* flower of a plant, the conclusion appears logical that in the

ovaries of the hybrids there are formed as many sorts of egg cells, and in the anthers as many sorts of pollen cells, as there are possible constant combination forms, and that these egg and pollen cells agree in their internal composition with those of the separate forms.

In point of fact it is possible to demonstrate theoretically that this hypothesis would fully suffice to account for the development of the hybrids in the separate generations, if we might at the same time assume that the various kinds of egg and pollen cells were formed in the hybrids on the average in equal numbers.

In order to bring these assumptions to an experimental proof, the following experiments were designed. Two forms which were constantly different in the form of the seed and the colour of the albumen were united by fertilization.

If the differentiating characters are again indicated as *A, B, a, b,* we have:

AB, seed parent;	*ab,* pollen parent;
A, form round;	*a,* form wrinkled;
B, albumen yellow.	*b,* albumen green.

The artificially fertilized seeds were sown together with several seeds of both original stocks, and the most vigorous examples were chosen for the reciprocal crossing. There were fertilized:

1. The hybrids with the pollen of *AB.*
2. The hybrids " " *ab.*
3. *AB* " " the hybrids.
4. *ab* " " the hybrids.

For each of these four experiments the whole of the flowers on three plants were fertilized. If the above theory be correct, there must be developed on the hybrids egg and pollen cells of the forms *AB, Ab, aB, ab,* and there would be combined:

1. The egg cells *AB, Ab, aB, ab* with the pollen cells *AB.*
2. The egg cells *AB, Ab, aB, ab* with the pollen cells *ab.*
3. The egg cells *AB* with the pollen cells *AB, Ab, aB, ab.*
4. The egg cells *ab* with the pollen cells *AB, Ab, aB, ab.*

From each of these experiments there could then result only the following forms:

1. *AB, ABb, AaB, AaBb.*
2. *AaBb, Aab, aBb, ab.*
3. *AB, ABb, AaB, AaBb.*
4. *AaBb, Aab, aBb, ab.*

If, furthermore, the several forms of the egg and pollen cells of the hybrids were produced on an average in equal numbers, then in each experiment the said four combinations should stand in the same ratio to

each other. A perfect agreement in the numerical relations was, however, not to be expected, since in each fertilization, even in normal cases, some egg cells remain undeveloped or subsequently die, and many even of the well-formed seeds fail to germinate when sown. The above assumption is also limited in so far that, while it demands the formation of an equal number of the various sorts of egg and pollen cells, it does not require that this should apply to each separate hybrid with mathematical exactness.

The first and second experiments had primarily the object of proving the composition of the hybrid egg cells, while the third and fourth experiments were to decide that of the pollen cells. As is shown by the above demonstration, the first and third experiments and the second and fourth experiments should produce precisely the same combinations, and even in the second year the result should be partially visible in the form and colour of the artificially fertilized seed. In the first and third experiments the dominant characters of form and colour, *A* and *B*, appear in each union, and are also partly constant and partly in hybrid union with the recessive characters *a* and *b*, for which reason they must impress their peculiarity upon the whole of the seeds. All seeds should therefore appear round and yellow, if the theory be justified. In the second and fourth experiments, on the other hand, one union is hybrid in form and in colour, and consequently the seeds are round and yellow; another is hybrid in form, but constant in the recessive character of colour, whence the seeds are round and green; the third is constant in the recessive character of form but hybrid in colour, consequently the seeds are wrinkled and yellow; the fourth is constant in both recessive characters, so that the seeds are wrinkled and green. In both these experiments there were consequently four sorts of seed to be expected—viz. round and yellow, round and green, wrinkled and yellow, wrinkled and green.

The crop fulfilled these expectations perfectly. There were obtained in the

1st Experiment, 98 exclusively round yellow seeds;
3rd " 94 " " " "

In the 2nd Experiment, 31 round and yellow, 26 round and green, 27 wrinkled and yellow, 26 wrinkled and green seeds.

In the 4th Experiment, 24 round and yellow, 25 round and green, 22 wrinkled and yellow, 27 wrinkled and green seeds.

There could scarcely be now any doubt of the success of the experiment; the next generation must afford the final proof. From the seed sown there resulted for the first experiment 90 plants, and for the third 87 plants which fruited: these yielded for the

1st Exp.	3rd Exp.		
20	25	round yellow seeds	*AB*
23	19	round yellow and green seeds	*ABb*
25	22	round and wrinkled yellow seeds . . .	*AaB*
22	21	round and wrinkled green and yellow seeds	*AaBb*

In the second and fourth experiments the round and yellow seeds yielded plants with round and wrinkled yellow and green seeds, *AaBb*.

From the round green seeds plants resulted with round and wrinkled green seeds, *Aab*.

The wrinkled yellow seeds gave plants with wrinkled yellow and green seeds, *aBb*.

From the wrinkled green seeds plants were raised which yielded again only wrinkled and green seeds, *ab*.

Although in these two experiments likewise some seeds did not germinate, the figures arrived at already in the previous year were not affected thereby, since each kind of seed gave plants which, as regards their seed, were like each other and different from the others. There resulted therefore from the

2nd Exp.	4th Exp.		
31	24	plants of the form	*AaBb*
26	25	" "	*Aab*
27	22	" "	*aBb*
26	27	" "	*ab*

In all the experiments, therefore, there appeared all the forms which the proposed theory demands, and they came in nearly equal numbers.

In a further experiment the characters of flower colour and length of stem were experimented upon, and selection was so made that in the third year of the experiment each character ought to appear in half of all the plants if the above theory were correct. *A, B, a, b* serve again as indicating the various characters.

A, violet-red flowers. *a*, white flowers.
B, axis long. *b*, axis short.

There subsequently appeared

The violet-red flower-colour	(*Aa*)	in 85	plants.
" white " "	(*a*)	in 81	"
" long stem	(*Bb*)	in 87	"
" short "	(*b*)	in 79	"

The theory adduced is therefore satisfactorily confirmed in this experiment also.

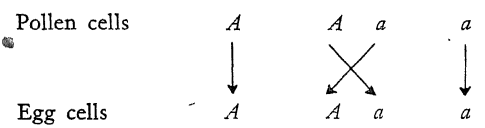
For the characters of form of pod, colour of pod, and position of flowers experiments were also made on a small scale, and results obtained in perfect agreement. All combinations which were possible through the union of the differentiating characters duly appeared, and in nearly equal numbers.

Experimentally, therefore, the theory is confirmed that *the pea hybrids form egg and pollen cells which, in their constitution, represent in equal numbers all constant forms which result from the combination of the characters united in fertilization.*

The difference of the forms among the progeny of the hybrids, as well as the respective ratios of the numbers in which they are observed, find a sufficient explanation in the principle above deduced. The simplest case is afforded by the developmental series of each pair of differentiating characters. This series is represented by the expression $A+2Aa+a$, in which A and a signify the forms with constant differentiating characters, and Aa the hybrid form of both. It includes in three different classes four individuals. In the formation of these, pollen and egg cells of the form A and a take part on the average equally in the fertilization; hence each form [occurs] twice, since four individuals are formed. There participate consequently in the fertilization

The pollen cells $A+A+a+a$
The egg cells $A+A+a+a$.

It remains, therefore, purely a matter of chance which of the two sorts of pollen will become united with each separate egg cell. According, however, to the law of probability, it will always happen, on the average of many cases, that each pollen form A and a will unite equally often with each egg cell form A and a, consequently one of the two pollen cells A in the fertilization will meet with the egg cell A and the other with an egg cell a, and so likewise one pollen cell a will unite with an egg cell A, and the other with egg cell a.



The result of the fertilization may be made clear by putting the signs for the conjoined egg and pollen cells in the form of fractions, those for the pollen cells above and those for the egg cells below the line. We then have

$$\frac{A}{A}+\frac{A}{a}+\frac{a}{A}+\frac{a}{a}.$$

In the first and fourth term the egg and pollen cells are of like kind, consequently the product of their union must be constant, viz. A and a; in the second and third, on the other hand, there again results a union of the two differentiating characters of the stocks, consequently the forms resulting from these fertilizations are identical with those of the hybrid from which they sprang. *There occurs accordingly a repeated hybridization.* This explains the striking fact that the hybrids are able to produce, besides the two parental forms, offspring which are like themselves; $\frac{A}{a}$ and $\frac{a}{A}$ both give the same union Aa, since, as already remarked above, it makes

no difference in the result of fertilization to which of the two characters the pollen or egg cells belong. We may write then

$$\frac{A}{A}+\frac{A}{a}+\frac{a}{A}+\frac{a}{a}=A+2Aa+a.$$

This represents the average result of the self-fertilization of the hybrids when two differentiating characters are united in them. In individual flowers and in individual plants, however, the ratios in which the forms of the series are produced may suffer not inconsiderable fluctuations. Apart from the fact that the numbers in which both sorts of egg cells occur in the seed vessels can only be regarded as equal on the average, it remains purely a matter of chance which of the two sorts of pollen may fertilize each separate egg cell. For this reason the separate values must necessarily be subject to fluctuations, and there are even extreme cases possible, as were described earlier in connection with the experiments on the form of the seed and the colour of the albumen. The true ratios of the numbers can only be ascertained by an average deduced from the sum of as many single values as possible; the greater the number the more are merely chance effects eliminated.

The developmental series for hybrids in which two kinds of differentiating characters are united contains among sixteen individuals nine different forms, viz.:

$$AB+Ab+aB+ab+2ABb+2aBb+2AaB+2Aab+4AaBb.$$

Between the differentiating characters of the original stocks *Aa* and *Bb* four constant combinations are possible, and consequently the hybrids produce the corresponding four forms of egg and pollen cells *AB, Ab, aB, ab,* and each of these will on the average figure four times in the fertilization, since sixteen individuals are included in the series. Therefore the participators in the fertilization are

Pollen cells $AB+AB+AB+AB+Ab+Ab+Ab+Ab+aB+aB+aB+aB+ab+ab+ab+ab.$

Egg cells $AB+AB+AB+AB+Ab+Ab+Ab+Ab+aB+aB+aB+aB+ab+ab+ab+ab.$

In the process of fertilization each pollen form unites on an average equally often with each egg cell form, so that each of the four pollen cells *AB* unites once with one of the forms of egg cell *AB, Ab, aB, ab.* In precisely the same way the rest of the pollen cells of the forms *Ab, aB, ab* unite with all the other egg cells. We obtain therefore

$$AB+ABb+AaB+AaBb+ABb+Ab+AaBb+Aab+AaB+AaBb+aB+aBb+AaBb+Aab+aBb+ab=AB+Ab+aB+ab+2ABb+2aBb+2AaB+2Aab+4AaBb.$$

In precisely similar fashion is the developmental series of hybrids exhibited when three kinds of differentiating characters are conjoined in them. The hybrids form eight various kinds of egg and pollen cells—*ABC, ABc, AbC, Abc, aBC, aBc, abC, abc*—and each pollen form unites itself again on the average once with each form of egg cell.

The law of combination of different characters which governs the development of the hybrids finds therefore its foundation and explanation in the principle enunciated, that the hybrids produce egg cells and pollen cells which in equal numbers represent all constant forms which result from the combinations of the characters brought together in fertilization.

CONCLUDING REMARKS

IT CAN HARDLY FAIL to be of interest to compare the observations made regarding *Pisum* with the results arrived at by the two authorities in this branch of knowledge, Kölreuter and Gärtner, in their investigations. According to the opinion of both, the hybrids in outward appearance present either a form intermediate between the original species or they closely resemble either the one or the other type, and sometimes can hardly be discriminated from it. From their seeds usually arise, if the fertilization was effected by their own pollen, various forms which differ from the normal type. As a rule, the majority of individuals obtained by one fertilization maintain the hybrid form, while some few others come more like the seed parent, and one or other individual approaches the pollen parent. This, however, is not the case with all hybrids without exception. Sometimes the offspring have more nearly approached, some the one and some the other of the two original stocks, or they all incline more to one or the other side; while in other cases *they remain perfectly like the hybrid* and continue constant in their offspring. The hybrids of varieties behave like hybrids of species, but they possess greater variability of form and a more pronounced tendency to revert to the original types.

With regard to the form of the hybrids and their development, as a rule an agreement with the observations made in *Pisum* is unmistakable. It is otherwise with the exceptional cases cited. Gärtner confesses even that the exact determination whether a form bears a greater resemblance to one or to the other of the two original species often involved great difficulty, so much depending upon the subjective point of view of the observer. Another circumstance could, however, contribute to render the results fluctuating and uncertain, despite the most careful observation and differentiation. For the experiments plants were mostly used which rank as good species and are differentiated by a large number of characters. In addition to the sharply defined characters, where it is a question of greater or less similarity, those characters must also be taken into account which are often difficult to define in words, but yet suffice, as every plant specialist knows, to give the forms a peculiar appearance. If it be accepted

that the development of hybrids follows the law which is valid for *Pisum,* the series in each separate experiment must contain very many forms, since the number of the terms, as is known, increases with the number of the differentiating characters as the powers of three. With a relatively small number of experimental plants the result therefore could only be approximately right, and in single cases might fluctuate considerably. If, for instance, the two original stocks differ in seven characters, and 100 and 200 plants were raised from the seeds of their hybrids to determine the grade of relationship of the offspring, we can easily see how uncertain the decision must become, since for seven differentiating characters the combination series contains 16,384 individuals under 2187 various forms; now one and then another relationship could assert its predominance, just according as chance presented this or that form to the observer in a majority of cases.

If, furthermore, there appear among the differentiating characters at the same time *dominant* characters, which are transmitted entire or nearly unchanged to the hybrids, then in the terms of the developmental series that one of the two original parents which possesses the majority of dominant characters must always be predominant. In the experiment described relative to *Pisum,* in which three kinds of differentiating characters were concerned, all the dominant characters belonged to the seed parent. Although the terms of the series in their internal composition approach both original parents equally, yet in this experiment the type of the seed parent obtained so great a preponderance that out of each sixty-four plants of the first generation fifty-four exactly resembled it, or only differed in one character. It is seen how rash it must be under such circumstances to draw from the external resemblances of hybrids conclusions as to their internal nature.

Gärtner mentions that in those cases where the development was regular, among the offspring of the hybrids the two original species were not reproduced, but only a few individuals which approached them. With very extended developmental series it could not in fact be otherwise. For seven differentiating characters, for instance, among more than 16,000 individuals—offspring of the hybrids—each of the two original species would occur only once. It is therefore hardly possible that these should appear at all among a small number of experimental plants; with some probability, however, we might reckon upon the appearance in the series of a few forms which approach them.

We meet with an *essential difference* in those hybrids which remain constant in their progeny and propagate themselves as truly as the pure species. According to Gärtner, to this class belong the *remarkably fertile hybrids Aquilegia atropurpurea canadensis, Lavatera pseudolbia thuringiaca, Geum urbano-rivale,* and some *Dianthus* hybrids; and, according to Wichura, the hybrids of the Willow family. For the history of the evolution of plants this circumstance is of special importance, since constant hybrids acquire the status of new species. The correctness of the facts is guaranteed by eminent observers, and cannot be doubted. Gärtner had

an opportunity of following up *Dianthus Armeria deltoides* to the tenth generation, since it regularly propagated itself in the garden.

With *Pisum* it was shown by experiment that the hybrids form egg and pollen cells of *different* kinds, and that herein lies the reason of the variability of their offspring. In other hybrids, likewise, whose offspring behave similarly we may assume a like cause; for those, on the other hand, which remain constant the assumption appears justifiable that their reproductive cells are all alike and agree with the foundation cell [fertilized ovum] of the hybrid. In the opinion of renowned physiologists, for the purpose of propagation one pollen cell and one egg cell unite in Phanerogams into a single cell, which is capable by assimilation and formation of new cells to become an independent organism. This development follows a constant law, which is founded on the material composition and arrangement of the elements which meet in the cell in a vivifying union. If the reproductive cells be of the same kind and agree with the foundation cell [fertilized ovum] of the mother plant, then the development of the new individual will follow the same law which rules the mother plant. If it chance that an egg cell unites with a *dissimilar* pollen cell, we must then assume that between those elements of both cells, which determine opposite characters, some sort of compromise is effected. The resulting compound cell becomes the foundation of the hybrid organism, the development of which necessarily follows a different scheme from that obtaining in each of the two original species. If the compromise be taken to be a complete one, in the sense, namely, that the hybrid embryo is formed from two similar cells, in which the differences are *entirely and permanently accommodated* together, the further result follows that the hybrids, like any other stable plant species, reproduce themselves truly in their offspring. The reproductive cells which are formed in their seed vessels and anthers are of one kind, and agree with the fundamental compound cell [fertilized ovum].

With regard to those hybrids whose progeny is *variable* we may perhaps assume that between the differentiating elements of the egg and pollen cells there also occurs a compromise, in so far that the formation of a cell as foundation of the hybrid becomes possible; but, nevertheless, the arrangement between the conflicting elements is only temporary and does not endure throughout the life of the hybrid plant. Since in the habit of the plant no changes are perceptible during the whole period of vegetation, we must further assume that it is only possible for the differentiating elements to liberate themselves from the enforced union when the fertilizing cells are developed. In the formation of these cells all existing elements participate in an entirely free and equal arrangement, by which it is only the differentiating ones which mutually separate themselves. In this way the production would be rendered possible of as many sorts of egg and pollen cells as there are combinations possible of the formative elements.

The attribution attempted here of the essential difference in the development of hybrids to *a permanent or temporary union* of the differing

cell elements can, of course, only claim the value of an hypothesis for which the lack of definite data offers a wide scope. Some justification of the opinion expressed lies in the evidence afforded by *Pisum* that the behaviour of each pair of differentiating characters in hybrid union is independent of the other differences between the two original plants, and, further, that the hybrid produces just so many kinds of egg and pollen cells as there are possible constant combination forms. The differentiating characters of two plants can finally, however, only depend upon differences in the composition and grouping of the elements which exist in the foundation cells [fertilized ova] of the same in vital interaction.

In conclusion, the experiments carried out by Kölreuter, Gärtner, and others with respect to *the transformation of one species into another by artificial fertilization* merit special mention. Particular importance has been attached to these experiments, and Gärtner reckons them among "the most difficult of all in hybridization."

If a species *A* is to be transformed into a species *B,* both must be united by fertilization and the resulting hybrids then be fertilized with the pollen of *B;* then, out of the various offspring resulting, that form would be selected which stood in nearest relation to *B* and once more be fertilized with *B* pollen, and so continuously until finally a form is arrived at which is like *B* and constant in its progeny. By this process the species *A* would change into the species *B.* Gärtner alone has effected thirty such experiments with plants of genera *Aquilegia, Dianthus, Geum, Lavatera, Lychnis, Malva, Nicotiana,* and *Oenothera.* The period of transformation was not alike for all species. While with some a triple fertilization sufficed, with others this had to be repeated five or six times, and even in the same species fluctuations were observed in various experiments. Gärtner ascribes this difference to the circumstance that "the specific [*typische*] power by which a species, during reproduction, effects the change and transformation of the maternal type varies considerably in different plants, and that, consequently, the periods within which the one species is changed into the other must also vary, as also the number of generations, so that the transformation in some species is perfected in more, and in others in fewer generations." Further, the same observer remarks "that in these transformation experiments a good deal depends upon which type and which individual be chosen for further transformation."

If it may be assumed that in these experiments the constitution of the forms resulted in a similar way to that of *Pisum,* the entire process of transformation would find a fairly simple explanation. The hybrid forms as many kinds of egg cells as there are constant combinations possible of the characters conjoined therein, and one of these is always of the same kind as that of the fertilizing pollen cells. Consequently there always exists the possibility with all such experiments that even from the second fertilization there may result a constant form identical with that of the pollen parent. Whether this really be obtained depends in each separate case upon the number of the experimental plants, as well as upon

the number of differentiating characters which are united by the fertilization. Let us, for instance, assume that the plants selected for experiment differed in three characters, and the species *ABC* is to be transformed into the other species *abc* by repeated fertilization with the pollen of the latter; the hybrids resulting from the first cross form eight different kinds of egg cells, viz.:

ABC, ABc, AbC, aBC, Abc, aBc, abC, abc.

These in the second year of experiment are united again with the pollen cells *abc,* and we obtain the series

AaBbCc+AaBbc+AabCc+aBbCc+Aabc+aBbc+abCc+abc.

Since the form *abc* occurs once in the series of eight terms, it is consequently little likely that it would be missing among the experimental plants, even were these raised in a smaller number, and the transformation would be perfected already by a second fertilization. If by chance it did not appear, then the fertilization must be repeated with one of those forms nearest akin, *Aabc, aBbc, abCc.* It is perceived that such an experiment must extend the farther *the smaller the number of experimental plants and the larger the number of differentiating characters* in the two original species; and that, furthermore, in the same species there can easily occur a delay of one or even of two generations such as Gärtner observed. The transformation of widely divergent species could generally only be completed in five or six years of experiment, since the number of different egg cells which are formed in the hybrid increases as the powers of two with the number of differentiating characters.

THE PERIODIC LAW

by

DMITRI IVANOVICH MENDELEYEV

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DMITRI IVANOVICH MENDELEYEV

1834–1907

DMITRI IVANOVICH MENDELEYEV lived in a Russia not familiar to the Western world. He was himself known, however, in scientific circles all over continental Europe; he visited England several times and the United States once. Westerners remembered his tall and slightly stooped figure, his deep-set, bright blue eyes, his finely modeled, gesturing hands, and his flowing hair—which he allowed a barber to cut only once a year, in the spring. They recalled him as "patriarchal," or as "a grand Russian of the province of Tver." His students at the Technological Institute in St. Petersburg and at the University of St. Petersburg remembered less his personal appearance than his talent for exposition in the lecture room and his more remarkable talent for stirring in students an ambition for knowledge. Several generations of them, including many eminent chemists and teachers, have paid tribute to his abilities. To them he said: "I do not wish to cram you with facts, but I want you to understand chemistry. And you should remember that hypotheses are not theories. By a theory I mean a conclusion drawn from the accumulated facts we now possess which enables us to foresee new facts which we do not yet know."

Mendeleyev's name is inseparably associated with the great generalization known as the periodic system of the elements. When he announced the generalization in 1869, it was, according to his own definition, not a hypothesis, but a theory. Subsequently, his own work and that of other chemists has given to the theory the validity of a law; and the Periodic Law is familiar to every student.

Mendeleyev became a teacher partly by accident. His father taught for many years, notably in the gymnasium at Tobolsk, Siberia, where he met and married Maria Dmitrievna. Soon after the birth of his youngest son, Dmitri, in

1834, he became wholly blind. He and his large family—there were then eight children surviving of the fourteen Maria had borne—had only his small pension to live on. But Maria had, like her famous son, intelligence and energy. As a girl, she had educated herself, in a time and a country where women were not schooled, by repeating the lessons assigned to her elder brother, Basil. With the same kind of directness, she now set about the re-establishment of a glassworks once owned by her family in Tobolsk. This she continued to manage and operate until after the death of her husband in 1847.

By this time young Dmitri had progressed through the classes of the Tobolsk gymnasium. He had also met some of the Decembrists in political exile in Tobolsk. They interested him so much in natural science that he and his mother decided that he should become a scientist. Some years later the dying Maria said to Dmitri, "Refrain from illusions, insist on work, and not on words. Patiently search divine and scientific truth." She had perhaps already phrased for herself these instructions for her scientist son when he graduated from the gymnasium. For then, not overcome by the death of her husband or by the calamity of the fire which destroyed her glassworks, she had gathered her scanty means and, with Dmitri and her daughter Elizabeth, had made the long journey to Moscow. Her design was to enter Dmitri at the university there, to make a scientist of him. Official difficulties blocked her way. After a year of effort, though she did not succeed in entering her son at the University of Moscow, she did procure government aid for his continued training at the Central Pedagogic Institute in St. Petersburg, under the Physico-Mathematical Faculty. The function of this school was to prepare teachers for the imperial schools. Maria lived just long enough to see her son complete his training and to graduate as a teacher.

Young Mendeleyev showed symptoms of lung disorder when he finished his course at the institute and was ordered south. Fortunately he obtained a post as chief science master in Simferopol, in the Crimea, and, during the Crimean War, another teaching post in Odessa. Residence in this southern climate cured his pulmonary trouble. In 1856, on his return to St. Petersburg, he took his master's degree in chemistry and became a *privatdocent* at the university. Three years later he was permitted to go to Paris, and later to Heidelberg for studies which he continued in St. Petersburg until he earned his doctor's degree in 1861. In 1866 he was appointed professor of general chemistry in the university, and retained his post there until a disagreement with the university administration forced his retirement in 1890. Three years later he was

appointed Director of the Bureau of Weights and Measures, and was still Director at his death in 1907.

Mendeleyev had a full life outside his profession. He twice married, reared a family of five children, took decided views on matters of education, art, and literature, wrote for journals and newspapers on controversial artistic subjects, and, though not a politician, showed himself a liberal in his political thinking. Yet the bulk of his energy he devoted to chemistry. Of his 262 printed publications, the majority are on chemical subjects and on industrial subjects dependent upon chemistry. His earliest paper dealt with the composition of some specimens of orthite. Presently he began a long examination of the physical properties of liquids and a series of experiments on the thermal expansion of liquids. In 1883 he announced as a result a simple expression for the expansion of liquids between 0° C. and the boiling point. By 1889 he had closed an extended series of studies on the densities of various solutions, he had studied the elasticity of gases minutely, he had published papers on the nitriles, on fractional distillation, on contact action, on the heat of combustion of organic substances, and on many other subjects in chemistry. He had also published papers of interest to mineralogists and to chemical geologists, had ascended in a balloon during the solar eclipse of 1887 to make observations of the upper atmosphere, and had so far developed his interest in and theories about petroleum that he had been commissioned by the government to investigate the oil industry at Baku and the naphtha springs in the Caucasus, and to study the operation of the oil fields in Pennsylvania.

Much of the experimental and observational work Mendeleyev accomplished in his career retains now only historical interest. Even his great book, the two-volume *Principles of Chemistry* (1869–71), for two generations the standard Russian textbook in chemistry, and a work several times translated into English, is outmoded in many chapters. It contains in Chapter XV his first full statement of the Periodic Law and his own account of the value and import of the law. This is the chapter here reprinted (from the English edition of 1897).

In his later years Mendeleyev maintained the simplicity of private life which had characterized his earlier days. He spent the greater part of the year in St. Petersburg, occasionally visiting his estates in Tver, where he carried on some agricultural experiments. He dined always at six, usually in the company of his family and friends, and he frequently spent the evening reading James Fenimore Cooper and Jules Verne. When he traveled, he chose to go third class so that he

might meet plain people. Had he chosen, he could have lived more elaborately. He had earned large sums by his industrial work, he had been honored by the Czar, he had been awarded the Davy Medal by the Royal Society, the Faraday Lectureship by the Chemical Society, and, in 1905, the Copley Medal. His renown as a teacher had attracted students from all over the world to his classes, and his fame as the discoverer of the Periodic Law had made his name familiar wherever scientific study flourished. By the time of his death his great generalization was acknowledged to be the most important chemical law put forward since the establishment of the atomic theory.

Though skilled and ingenious in experiment, Mendeleyev's perspicacity made him pre-eminent in theoretical work. Others' experiments and discoveries have confirmed his theory; it continues to be a lasting influence in all research in chemistry.

THE PERIODIC LAW

THE GROUPING OF THE ELEMENTS AND THE PERIODIC LAW

THE SUM of the data concerning the chemical transformations proper to the elements (for instance, with respect to the formation of acids, salts, and other compounds having definite properties) is insufficient for accurately determining the relationship of the elements, inasmuch as this may be many-sided. Thus, lithium and barium are in some respects analogous to sodium and potassium, and in others to magnesium and calcium. It is evident, therefore, that for a complete judgment it is necessary to have, not only qualitative, but also quantitative, exact and measurable, data. When a property can be measured it ceases to be vague, and becomes quantitative instead of merely qualitative.

Among these measurable properties of the elements, or of their corresponding compounds, are: (*a*) isomorphism, or the analogy of crystalline forms; and, connected with it, the power to form crystalline mixtures which are isomorphous; (*b*) the relation of the volumes of analogous compounds of the elements; (*c*) the composition of their saline compounds; and (*d*) the relation of the atomic weights of the elements. In this chapter we shall briefly consider these four aspects of the matter, which are exceedingly important for a natural and fruitful grouping of the elements, facilitating, not only a general acquaintance with them, but also their detailed study.

Historically the first, and an important and convincing, method for finding a relationship between the compounds of two different elements is by *isomorphism*. This conception was introduced into chemistry by Mitscherlich (in 1820), who demonstrated that the corresponding salts of arsenic acid, H_3AsO_4, and the phosphoric acid, H_3PO_4, crystallise with an equal quantity of water, show an exceedingly close resemblance in crystalline form (as regards the angles of their faces and axes), and are able to crystallise together from solutions, forming crystals containing a mixture of the isomorphous compounds. Isomorphous substances are those which, with an equal number of atoms in their molecules, present an analogy in their chemical reactions, a close resemblance in their properties, and a similar or very nearly similar crystalline form: they often contain certain elements in common, from which it is to be concluded that the remaining elements (as in the preceding example of As and P) are analogous to each other. And inasmuch as crystalline forms are capable of exact measurement, the external form, or the relation of the molecules which causes

their grouping into a crystalline form, is evidently as great a help in judging of the internal forces acting between the atoms as a comparison of reactions, vapour densities, and other like relations. It will be sufficient to call to mind that the compounds of the alkali metals with the halogens RX, in a crystalline form, all belong to the cubic system and crystallise in octahedra or cubes—for example, sodium chloride, potassium chloride, potassium iodide, rubidium chloride, &c. The nitrates of rubidium and cæsium appear in anhydrous crystals of the same form as potassium nitrate. The carbonates of the metals of the alkaline earths are isomorphous with calcium carbonate—that is, they either appear in forms like calc-spar or in the rhombic system in crystals analogous to aragonite. Furthermore, sodium nitrate crystallises in rhombohedra, closely resembling the rhombohedra of calc-spar (calcium carbonate), $CaCO_3$, whilst potassium nitrate appears in the same form as aragonite, $CaCO_3$, and the number of atoms in both kinds of salts is the same. They all contain one atom of a metal (K, Na, Ca), one atom of a non-metal (C, N), and three atoms of oxygen. The analogy of form evidently coincides with an analogy of atomic composition. But there is not any close resemblance in their properties. It is evident that calcium carbonate approaches more nearly to magnesium carbonate than to sodium nitrate, although their crystalline forms are all equally alike. Isomorphous substances which are perfectly analogous to each other are not only characterised by a close resemblance of form (homeomorphism), but also by the faculty of entering into analogous reactions, which is not the case with RNO_3 and RCO_3. The most important and direct method of recognising perfect isomorphism—that is, the absolute analogy of two compounds—is given by that property of analogous compounds of separating from solutions *in homogeneous crystals, containing the most varied proportions* of the analogous substances which enter into their composition. These quantities do not seem to be in dependence on the molecular or atomic weights, and if they are governed by any laws they must be analogous to those which apply to indefinite chemical compounds. This will be clear from the following examples. Potassium chloride and potassium nitrate are not isomorphous with each other, and are in an atomic sense composed in a different manner. If these salts be mixed in a solution and the solution be evaporated, independent crystals of the two salts will separate, each in that crystalline form which is proper to it. The crystals will not contain a mixture of the two salts. But if we mix the solutions of two isomorphous salts together, then, under certain circumstances, crystals will be obtained which contain both these substances. However, this cannot be taken as an absolute rule, for if we take a solution saturated at a high temperature with a mixture of potassium and sodium chlorides, then on evaporation sodium chloride only will separate, and on cooling only potassium chloride. The first will contain very little potassium chloride, and the latter very little sodium chloride. But if we take, for example, a mixture of solutions of magnesium sulphate and zinc sulphate, they cannot be separated from each other by evaporating the mixture, notwithstanding the rather considerable differ-

ence in the solubility of these salts. Again, the isomorphous salts, magnesium carbonate, and calcium carbonate are found together—that is, in one crystal—in nature. The angle of the rhombohedron of these magnesia-lime spars is intermediate between the angles proper to the two spars individually (for calcium carbonate, the angle of the rhombohedron is 105° 8′; magnesium carbonate, 107° 30′; $CaMg(CO_3)_2$, 106° 10′). Certain of these *isomorphous mixtures* of calc and magnesia spars appear in well-formed crystals, and in this case there not unfrequently exists a simple molecular proportion of strictly definite chemical combination between the component salts—for instance, $CaCO_3,MgCO_3$—whilst in other cases, especially in the absence of distinct crystallisation (in dolomites), no such simple molecular proportion is observable: this is also the case in many artificially prepared isomorphous mixtures. The microscopical and crystallo-optical researches of Professor Inostrantzoff and others show that in many cases there is really a mechanical, although microscopically minute, juxtaposition in one whole of the heterogeneous crystals of calcium carbonate (double refracting) and of the compound $CaMgC_2O_6$. If we suppose the adjacent parts to be microscopically small (on the basis of the researches of Mallard, Weruboff, and others), we obtain an idea of isomorphous mixtures. A formula of the following kind is given to isomorphous mixtures: for instance, for spars, RCO_3, where R=Mg, Ca, and where it may be Fe, Mn . . . , &c. This means that the Ca is partially replaced by Mg or another metal. Alums form a common example of the separation of isomorphous mixtures from solutions. They are double sulphates (or seleniates) of alumina (or oxides isomorphous with it) and the alkalis, which crystallise in well-formed crystals. If aluminium sulphate be mixed with potassium sulphate, an alum separates, having the composition $KAlS_2O_8,12H_2O$. If sodium sulphate or ammonium sulphate, or rubidium (or thallium) sulphate be used, we obtain alums having the composition $RAlS_2O_8,12H_2O$. Not only do they all crystallise in the cubic system, but they also contain an equal atomic quantity of water or crystallisation ($12H_2O$). Besides which, if we mix solutions of the potassium and ammonium ($NH_4AlS_2O_8,12H_2O$) alums together, then the crystals which separate will contain various proportions of the alkalis taken, and separate crystals of the alums of one or the other kind will not be obtained, but each separate crystal will contain both potassium and ammonium. Nor is this all; if we take a crystal of a potassium alum and immerse it in a solution capable of yielding ammonia alum, the crystal of the potash alum will continue to grow and increase in size in this solution—that is, a layer of the ammonia or other alum will deposit itself upon the planes bounding the crystal of the potash alum. This is very distinctly seen if a colourless crystal of a common alum be immersed in a saturated violet solution of chrome alum, $KCrS_2O_8,12H_2O$, which then deposits itself in a violet layer over the colourless crystal of the alumina alum, as was observed even before Mitscherlich noticed it. If this crystal be then immersed in a solution of an alumina alum, a layer of this salt will form over the layer of chrome alum, so that one alum is able to incite the growth of the other.

If the deposition proceed simultaneously, the resultant intermixture may be minute and inseparable, but its nature is understood from the preceding experiments; the attractive force of crystallisation of isomorphous substances is so nearly equal that the attractive power of an isomorphous substance induces a crystalline superstructure exactly the same as would be produced by the attractive force of like crystalline particles. From this it is evident that one isomorphous substance may *induce the crystallisation* of another. Such a phenomenon explains, on the one hand, the aggregation of different isomorphous substances in one crystal, whilst, on the other hand, it serves as a most exact indication of the nearness both of the molecular composition of isomorphous substances and of those forces which are proper to the elements which distinguish the isomorphous substances. Thus, for example, ferrous sulphate or green vitriol crystallises in the monoclinic system and contains seven molecules of water, $FeSO_4,7H_2O$, whilst copper vitriol crystallises with five molecules of water in the triclinic system, $CuSO_4,5H_2O$; nevertheless, it may be easily proved that both salts are perfectly isomorphous; that they are able to appear in identically the same forms and with an equal molecular amount of water. For instance, Marignac, by evaporating a mixture of sulphuric acid and ferrous sulphate under the receiver of an air pump, first obtained crystals of the hepta-hydrated salt, and then of the penta-hydrated salt $FeSO_4,5H_2O$, which were perfectly similar to the crystals of copper sulphate. Furthermore, Lecoq de Boisbaudran, by immersing crystals of $FeSO_4,7H_2O$ in a supersaturated solution of copper sulphate, caused the latter to deposit in the same form as ferrous sulphate, in crystals of the monoclinic system, $CuSO_4,7H_2O$.

Hence it is evident that isomorphism—that is, the analogy of forms and the property of inducing crystallisation—may serve as a means for the discovery of analogies in molecular composition. We will take an example in order to render this clear. If, instead of aluminium sulphate, we add magnesium sulphate to potassium sulphate, then, on evaporating the solution, the double salt $K_2MgS_2O_8,6H_2O$ separates instead of an alum, and the ratio of the component parts (in alums one atom of potassium per $2SO_4$, and here two atoms) and the amount of water of crystallisation (in alums 12, and here 6 equivalents per $2SO_4$) are quite different; nor is this double salt in any way isomorphous with the alums, nor capable of forming an isomorphous crystalline mixture with them, nor does the one salt provoke the crystallisation of the other. From this we must conclude that although alumina and magnesia, or aluminium and magnesium, resemble each other, they are not isomorphous, and that although they give partially similar double salts, these salts are not analogous to each other. And this is expressed in their chemical formulæ by the fact that the number of atoms in alumina or aluminium oxide, Al_2O_3, is different from the number in magnesia, MgO. Aluminium is trivalent and magnesium bivalent. Thus, having obtained a double salt from a given metal, it is possible to judge of the analogy of the given metal with aluminium or with magnesium, or of the absence of such an analogy, from the composition and

TABLE I

Distribution of the Elements in Groups and Series

Group	I	II	III	IV	V	VI	VII	VIII
Series 1 . . .	. H	—	—	—	—	—	—	
" 2 . . .	Li .	Be .	B .	C .	N .	O .	F .	
" 3 . . .	. Na	. Mg	. Al	. Si	. P	. S	. Cl	
" 4 . . .	K .	Ca .	Sc .	Ti .	V .	Cr .	Mn .	Fe . Co . Ni . Cu
" 5 . . .	. (Cu)	. Zn	. Ga	. Ge	. As	. Se	. Br	
" 6 . . .	Rb .	Sr .	Y .	Zr .	Nb .	Mo .	— .	Ru . Rh . Pd . Ag
" 7 . . .	. (Ag)	. Cd	. In	. Sn	. Sb	. Te	. I	
" 8 . . .	Cs .	Ba .	La .	Ce .	Di? .	— .	— .	— — — —
" 9 . . .	. —	. —	. —	. —	. —	. —	. —	
" 10 . . .	— .	— .	Yb .	— .	Ta .	W .	— .	Os . Ir . Pt . Au
" 11 . . .	. (Au)	Hg	. Tl	. Pb	. Bi	. —	. —	
" 12 . . .	— .	— .	— .	Th .	— .	U .	— .	
	R_2O	R_2O_2	R_2O_3	R_2O_4	R_2O_5	R_2O_6	R_2O_7	Higher oxides
	—	RO	—	RO_2	—	RO_3	—	RO_4
	—	—	—	RH_4	RH_3	RH_2	RH	Hydrogen compounds

form of this salt. Thus zinc, for example, does not form alums, but forms a double salt with potassium sulphate, which has a composition exactly like that of the corresponding salt of magnesium. It is often possible to distinguish the bivalent metals analogous to magnesium or calcium from the trivalent metals, like aluminium, by such a method. Furthermore, the specific heat and vapour density serve as guides. There are also indirect proofs. Thus iron gives ferrous compounds, FeX_2, which are isomorphous with the compounds of magnesium, and ferric compounds, FeX_3, which are isomorphous with the compounds of aluminium; in this instance the relative composition is directly determined by analysis, because, for a given amount of iron, $FeCl_2$ only contains two thirds of the amount of chlorine which occurs in $FeCl_3$, and the composition of the corresponding oxygen compounds, *i.e.* of ferrous oxide, FeO, and ferric oxide, Fe_2O_3, clearly indicates the analogy of the ferrous oxide with MgO and of the ferric oxide with Al_2O_3.

Thus in the building up of similar molecules in crystalline forms we see one of the numerous means for judging of the internal world of molecules and atoms, and one of the weapons for conquests in the invisible world of molecular mechanics which forms the main object of physico-chemical knowledge. This method has more than once been employed for discovering the analogy of elements and of their compounds; and as crystals are measurable, and the capacity to form crystalline mixtures can be experimentally verified, this method is a numerical and measurable one, and in no sense arbitrary.

The regularity and simplicity expressed by the exact laws of crystalline form repeat themselves in the aggregation of the atoms to form molecules. Here, as there, there are but few forms which are essentially different, and their apparent diversity reduces itself to a few fundamental differences of type. There the molecules aggregate themselves into crystalline forms; here, the atoms aggregate themselves into molecular forms or into *the types of compounds*. In both cases the fundamental crystalline or molecular forms are liable to variations, conjunctions, and combinations. If we know that potassium gives compounds of the fundamental type KX, where X is a univalent element (which combines with one atom of hydrogen, and is, according to the law of substitution, able to replace it), then we know the composition of its compounds: K_2O, KHO, KCl, NH_2K, KNO_3, K_2SO_4, $KHSO_4$, $K_2Mg\ (SO_4)_2{,}6H_2O$, &c. All the possible derivative crystalline forms are not known. So also all the atomic combinations are not known for every element. Thus in the case of potassium, KCH_3, K_3P, K_2Pt, and other like compounds which exist for hydrogen or chlorine, are unknown.

Only a few fundamental types exist for the building up of atoms into molecules, and the majority of them are already known to us. If X stands for a univalent element, and R for an element combined with it, then eight atomic types may be observed:—

$$RX,\ RX_2,\ RX_3,\ RX_4,\ RX_5,\ RX_6,\ RX_7,\ RX_8.$$

Let X be chlorine or hydrogen. Then as examples of the first type we have: H_2, Cl_2, HCl, KCl, NaCl, &c. The compounds of oxygen or calcium may serve as examples of the type RX_2: OH_2, OCl_2, OHCl, CaO, $Ca(OH)_2$, $CaCl_2$, &c. For the third type RX_3 we know the representative NH_3 and the corresponding compounds N_2O_3, NO(OH), NO(OK), PCl_3, P_2O_3, PH_3, SbH_3, Sb_2O_3, B_2O_3, BCl_3, Al_2O_3, &c. The type RX_4 is known among the hydrogen compounds. Marsh gas, CH_4, and its corresponding saturated hydrocarbons, C_nH_{2n+2}, are the best representatives. Also CH_3Cl, CCl_4, $SiCl_4$, $SnCl_4$, SnO_2, CO_2, SiO_2 and a whole series of other compounds come under this class. The type RX_5 is also already familiar to us, but there are no purely hydrogen compounds among its representatives. Sal-ammoniac, NH_4Cl, and the corresponding $NH_4(OH)$, $NO_2(OH)$, $ClO_2(OK)$, as well as PCl_5, $POCl_3$, &c., are representatives of this type. In the higher types also there are no hydrogen compounds, but in the type RX_6 there is the chlorine compound WCl_6. However, there are many oxygen compounds, and among them SO_3 is the best known representative. To this class also belong $SO_2(OH)_2$, SO_2, Cl_2, $SO_2(OH)Cl$, CrO_3, &c., all of an acid character. Of the higher types there are in general only oxygen and acid representatives. The type RX_7 we know in perchloric acid, $ClO_3(OH)$, and potassium permanganate, $MnO_3(OK)$, is also a member. The type RX_8 in a free state is very rare; osmic anhydride, OsO_4, is the best known representative of it.

The four lower types RX, RX_2, RX_3, and RX_4 are met with in compounds of the elements R with chlorine and oxygen, and also in their compounds with hydrogen, whilst the four higher types only appear for such acid compounds as are formed by chlorine, oxygen, and similar elements.

Among the oxygen compounds the *saline oxides* which are capable of forming salts either through the function of a base or through the function of an acid anhydride attract the greatest interest in every respect. Certain elements, like calcium and magnesium, only give one saline oxide—for example, MgO, corresponding with the type MgX_2. But the majority of the elements appear in several such forms. Thus copper gives CuX and CuX_2, or Cu_2O and CuO. If an element R gives a higher type RX_n, then there often also exist, as if by symmetry, lower types, RX_{n-2}, RX_{n-4}, and in general such types as differ from RX_n by an even number of X. Thus in the case of sulphur the types SX_2, SX_4, and SX_6 are known—for example SH_2, SO_2, and SO_3. The last type is the highest, SX_6. The types SX_5 and SX_3 do not exist. But even and uneven types sometimes appear for one and the same element. Thus the types RX and RX_2 are known for copper and mercury.

Among the *saline* oxides only the *eight types* enumerated below are known to exist. They determine the possible formulæ of the compounds of the elements, if it be taken into consideration that an element which gives a certain type of combination may also give lower types. For this reason the rare type of the *suboxides* or quaternary oxides R_4O (for instance, Ag_4O, Ag_2Cl) is not characteristic; it is always accompanied by

TABLE II

The periodic dependence of the composition of the simplest compounds and properties of the simple bodies upon the atomic weights of the elements

Molecular composition of the higher hydrogen and metallo-organic compounds				Atomic weights of the elements			Composition of the saline compounds, X=Cl								Peroxides	Lower hydrogen compounds	Simple bodies		
							Br, (NO_2), ½ O, ½ (SO_4) OH, (OM)=Z, where M=K, ½ Ca, ⅓ Al, &c.										Sp. gr.	Sp. vol.	Melting point
E=CH_3, C_2H_5, &c.							Form Oxides R_2O	RX RO	RX_2 R_2O_3	RX_3 RO_2	RX_4 R_2O_5	RX_5 RO_3	RX_6 R_2O_7	RX_7 RO_4	RX_8				
[1]	[2]	[3]	[4]	[5]	[6]		[7]	[8]	[9]	[10]	[11]	[12]	[13]	[14]	[15]	[16]	[17]	[18]	[19]
			HH	H	1,005	(mean)	HX or H_2O								H_2O_2	—	*0·05	20	−250°?
				Li	7·02	(Stas)	LiX								—	—	0·59	11·9	180°
	BE_2	—	—	Be	9·1	(Nilson Pettersson) .	—	BeX_2							—	BeH	1·64	5·5	900°?
				B	11·0	(Ramsay Ashton) .	—	—	BX_3						—	—	2·5	4·4	1,300°?
CH_4	C_2H_6	C_2H_4	C_2H_2	C	12·00	(Roscoe)	—	CO	—	COZ_2					C_2O_5*	—	*1·9	6·3	2,600°?
	NH_3	N_2H_4	—	N	14·04	(Stas)	N_2O	NO	NOZ	NO_2	NO_2Z				N_2O_5*	N_3H	*0·6	23	−203°
		OH_2	—	O	16	(conventional) . . .	—	OX_2							O_3	—	*0·9	18	−230°?
			FH	F	19·0	(Christiansen) . . .	FZ								—	—	?1·0	19	?
			NaE	Na	23·04	(Stas)	NaX								Na_2O	Na_2H	0·98	23·5	96°
		MgE_2	—	Mg	24·3	(Burton)	—	MgX_2							—	MgH	1·74	14	500°
	AlE_3	—	—	Al	27·1	(Mallet)	—	—	AlX_3						—	—	2·6	11	600°
SiH_4	Si_2E_6	—	—	Si	28·4	(Thorpe Young) . .	—	—	—	$SiOZ_2$					—	—	2·3	12	1,300°?
	PH_3	P_2H_4	—	P	31·0	(v. d. Plaats)	—	—	PX_3	—	POZ_3				—	P_2H	2·2	14	44°
		SH_2	—	S	32·06	(Stas)	—	SX_2	—	SOZ_2	—	SO_2Z_2			S_2O_7	—	2·07	15	114°
			ClH	Cl	35·45	(Stas)	ClZ	—	ClOZ	—	ClO_2Z	—	ClO_3Z		—	—	*1·3	27	−75°
				K	39·15	(Stas)	KX								KO_2	K_2H	0·87	45	58°
				Ca	40·0	(Dumas)	—	CaX_2							CaO_2	CaH	1·56	26	800°
				Sc	44·0	(Nilson) . . .	—	—	ScX_3						—	—	?2·5	?18	1,200°?
				Ti	48·1	(Thorpe)	—	TiX_2	TiX_3	TiX_4					TiO_3	—	3·6	13	2,500°?
				V	51·2	(Roscoe)	—	VO	VOX	—	VOZ_3				—	—	5·5	9	3,000°?
				Cr	52·1	(Rawson)	—	CrX_2	CrX_3	CrO_2	—	CrO_2Z_2			Cr_2O_7	—	6·7	7·7	2,000°?
				Mn	55·1	(Marignac)	—	MnX_2	MnX_3	MnO_2	—	MnO_2Z_2	MnO_3Z		—	—	7·5	7·3	1,500°
				Fe	56·0	(Dumas)	—	FeX_2	FeX_3	—	—	FeO_2Z_2			—	Fe_nH_*	7·8	7·2	1,450°
				Co	58·9	(Zimmermann) . . .	—	CoX_2	CoX_3	CoO_2					—	—	8·6	6·8	1,400°
				Ni	59·4	(Winkler)	—	NiX_2	NiX_3						—	Ni_nH	8·7	6·8	1,350°
				Cu	63·6	(Richards)	CuX	CuX_2							Cu_2O_5*	CuH	8·8	7·2	1,054°
		ZnE_2	—	Zn	65·3	(Marignac)	—	ZnX_2							ZnO_2*	—	7·1	9·2	418°
	GaE_3	—	—	Ga	69·9	(Boisbaudran) . . .	—	—	GaX_3						—	—	5·96	11·7	30°
GeE_4	—	—	—	Ge	72·3	(Winkler)	—	GeX_2	—	GeX_4					—	—	5·47	13·2	900°
	AsH_3	—	—	As	75·0	(Dumas)	—	AsS	AsX_3	AsS_2	AsO_2Z				—	As_4H_*	5·65	13·3	500°

		SeH_2	—	Se	79·0†	(Pettersson) . . .	—	—	—	$SeOZ_2$	—	ScO_2Z_2			—	—	4·8	16	217°
			BrH	Br	79·95	(Stas)	BrZ	—	BrOZ	—	BrO_2Z		BrO_3Z		—	—	3·1	26	. —7°
				Rb	85·5	(Godeffroy)	RbX								RbO	Rb_2H_*	1·5	57	39°
				Sr	87·6	(Dumas)	—	SrX_2							SrO_2	SrH*	2·5	35	600°?
				Y	89	(Cleve)	—	—	YX_3						—	—	*3·4	*26	1,000°?
				Zr	90·6	(Bailey)	—	—	—	ZrX_4					—	Zr_nH_*	4·1	22	1,500°?
				Nb	94	(Marignac).	—	—	NbX_3	—	NbO_2Z				—	Nb_nH_*	7·1	13	1,800°?
				Mo	96·1	(Maas)	—	—	MoX_3	MoX_4	—	MoO_2Z_2			Mo_2O_7	—	8·6	11	2,200°?
						Unknown metal (eka-manganese, Em=99).							EmO_3Z		—	—	—	—	—
				Ru	101·7	(Joly)	—	RuX_2	RuX_3	RuX_4	—	RuO_2Z_2	RuO_3Z	RuO_4	—	Ru_nH_*	12·2	8·4	2,000°?
				Rh	102·7	(Seubert).	—	RhX_2	RhX_3	RhX_4	—	RhO_2Z_2			—	Rh_nH_*	12·1	8·6	1,900°?
				Pd	106·4	(Keller Smith) . . .	PdX	PdX_2	—	PdX_4					—	PdH_2	11·4	8·3	1,500°
				Ag	107·92	(Stas)	AgX								AgO	—	10·5	10·3	950°
			CdH	Cd	112·1	(Lorimer Smith) . .	—	CdX_2							CdO_2	—	8·6	13	320°
		InH	—	In	113·6	(Winkler)	—	InX_2	InX_3						—	—	7·4	14	176°
	SnE_4	—	—	Sn	119·1	(Classen).	—	SnX_2	—	SnX_4					SnO_2	—	7·2	16	232°
		SbH_3	—	Sb	120·4	(Schneider).	—	—	SbX_3	—	SbO_2Z				—	—	6·7	18	432°
			TeH_2	Te	125·1	(Brauner)	—	—	—	$TeOZ_2$	—	TeO_2Z_2			—	—	6·4	20	455°
			IH	I	126·85	(Stas)	JZ	—	IZ_3	—	IO_2Z	—	IO_3Z		—	—	4·9	26	114°
				Cs	132·7	(Godeffroy)	CsX								—	Cs_2H_*	2·37	56	27°
				Ba	137·4	(Richards)	—	BaX_2							BaO_2	BaH*	3·75	36	?
				La	138·2	(Brauner)	—	—	LaX_3						—	—	6·1	23	?
				Ce	140·2	(Brauner)	—	—	CeX_3	CeX_4					—	—	6·6	21	700°?
						Little known Di=142.1 and Yb=173.2, and over 15 unknown elements.													
				Ta	182·7	(Marignac).	—	—	—	—	TaO_2Z				—	Ta_nH_*	10·4	18	?
				W	184·0	(Waddel)	—	—	—	WX_4	—	WO_2Z_2			W_2O_7	—	19·1	9·6	2,600°
						Unknown element.													
				Os	191·6	(Seubert).	—	—	OsX_3	OsX_4	—	OsO_2Z_2	—	OsO_4	—	—	22·5	8·5	2,700°?
				Ir	193·3	(Joly)	—	—	IrX_3	IrX_4	—	IrO_2Z_2			—	Ir_nH_*	22·4	8·6	2,000°
				Pt	196·0	(Dittmar McArthur)	—	PtX_2	—	PtX_4					—	Pt_nH_*	21·4	9·2	1,775°
				Au	197·5	(Mallet)	AuX	—	AuX_3						—	—	19·3	10	1,045°
			HgH	Hg	200·5	(Erdmann Mar.) . .	HgX	HgX_2							—	—	13·6	15	—39°
		TlH	—	Tl	204·1	(Crookes)	TlX	—	TlX_3						—	—	11·8	17	294°
	PbE_4	—	—	Pb	206·90	(Stas)	—	PbX_2	—	$PbOZ_2$					—	—	11·3	18	328°
		BiE_3	—	Bi	208·9	(Classen).	—	—	BiX_3	—	BiO_2Z				—	—	9·8	21	269°
						Five unknown elements.													
				Th	232·4	(Krüss Nilson) . . .	—	—	—	ThX_4					—	—	11·1	21	?
						Unknown element.													
				U	239·3	(Zimmermann) . . .	—	—	—	UO_2	—	UO_2X_2			UO_4	—	18 7	13	2,400°?

[*See notes on following page.*]

†From analogy there is reason for thinking that the atomic weight of selenium is really slightly less than 79·0.

Columns 1, 2, 3, and 4 give the molecular composition of the hydrogen and metallo-organic compounds, exhibiting the most characteristic forms assumed by the elements. The first column contains only those which correspond to the form RX_4, the second column those of the form RX_3, the third of the form RX_2, and the fourth of the form RX, so that the periodicity stands out clearly (*see* Column 16).

Column 5 contains the symbols of all the more or less well-known elements, placed according to the order of the magnitude of their atomic weights.

Column 6 contains the atomic weights of the elements according to the most trustworthy determinations. The names of the investigators are given in parenthesis. The atomic weight of oxygen, taken as 16, forms the basis upon which these atomic weights were calculated. Some of these have been recalculated by me on the basis of Stas's most trustworthy data.

Columns 7–14 contain the composition of the saline compounds of the elements, placed according to their forms, RX, RX_2 to RX_8 (in the 14th column). If the element R has a metallic character like H, Li, Be, &c., then X represents Cl, NO_3, $\frac{1}{2}SO_4$, &c., haloid radicles, or (OH) if a perfect hydrate is formed (alkali, aqueous base), or $\frac{1}{2}O$, $\frac{1}{2}S$, &c. when an anhydrous oxide, sulphide, &c. is formed. For instance, NaCl, $Mg(NO_3)_2$, $Al_2(SO_4)_3$, correspond to NaX, MgX_2, and AlX_3; so also $Na(OH)$, $Mg(OH)_2$, $Al(OH)_3$, Na_2O, MgO, Al_2O_3, &c. But if the element, like C or N, be of a metalloid or acid character, X must be regarded as (OH) in the formation of hydrates; (OM) in the formation of salts, where M is the equivalent of a metal, $\frac{1}{2}O$ in the formation of an anhydride, and Cl in the formation of a chloranhydride; and in this case (*i.e.* in the acid compounds) Z is put in the place of X; for example, the formulae COZ_2, NO_2Z, MnO_3Z, FeO_2Z_2, and IZ_3 correspond to $CO(\overset{\circ}{N}aO)_2=Na_2CO_3$, $COCl_2$, CO_2, $NO_2(NaO)=NaNO_3$, NO_2Cl, $NO_2(OH)=HNO_3$; $MnO_3(OK)=KMnO_4$, ICl, &c.

The 15th column gives the compositions of the peroxides of the elements, *taking them as anhydrous*. An asterisk (*) is attached to those of which the composition has not been well established, and a dash (—) shows that for a given element no peroxides have yet been obtained. The peroxides contain more oxygen than the higher saline oxides of the same elements, are powerfully oxidising, and easily give peroxide of hydrogen. This latter circumstance necessitates their being referred to the type of peroxide of hydrogen, if bases and acids are referred to the type of water.

The 16th column gives the composition of the lower hydrogen compounds like N_3H and Na_2H. They may often be regarded as alloys of hydrogen, which is frequently disengaged by them at a comparatively moderate temperature. They differ greatly in their nature from the hydrogen compounds given in columns 1–4.

Column 17 gives the specific gravity of the elements in a solid and a liquid state. An asterisk (*) is placed by those which can either only be assumed from analogy (for example, the sp. gr. of fluorine and hydrogen, which have not been obtained in a liquid state), or which vary very rapidly with a variation of temperature and pressure (like oxygen and nitrogen), or physical state (for instance, carbon in passing from the state of charcoal to graphite and diamond). But as the sp. gr. in general varies with the temperature, mechanical condition, &c., the figures given, although chosen from the most trustworthy sources, can only be regarded as approximate, and not as absolutely true. They clearly show a certain periodicity; for instance, the sp. gr. diminishes from Al on both sides (Al, Mg, Na, with decreasing atomic weight; and Al, Si, P, S, Cl, with increasing atomic weight), it also diminishes on both sides from Cu, Ru, and Os.

The same remarks refer to the figures in the 18th column, which gives the so-called atomic volumes of the simple bodies, or the quotient of their atomic weight and specific gravity. For Na, K, Rb, and Cs the atomic volume is greatest among the neighbouring elements. For Ni, Pd, and Os it is least, and this indicates the periodicity of this property of the simple bodies.

The last (19th) column gives the melting points of the simple bodies. Here also a periodicity is seen, *i.e.* a maximum and minimum value between which there are intermediate values, as we see, for instance, in the series Cl, K, Ca, Se, and Ti, or in the series Cr, Mn, Fe, Co, Ni, Cu, Zn, Ga, and Ge.

one of the higher grades of oxidation, and the compounds of this type are distinguished by their great chemical instability, and split up into an element and the higher compound (for instance, $Ag_4O=2Ag+Ag_2O$). Many elements, moreover, form transition oxides whose composition is intermediate, which are able, like N_2O_4, to split up into the lower and higher oxides. Thus iron gives magnetic oxide, Fe_3O_4, which is in all respects (by its reactions) a compound of the suboxide FeO with the oxide Fe_2O_3. The independent and more or less stable saline compounds correspond with the following eight types:—

R_2O; salts RX, hydroxides ROH. Generally basic like K_2O, Na_2O, Hg_2O, Ag_2O, Cu_2O; if there are acid oxides of this composition they are very rare, are only formed by distinctly acid elements, and even then have only feeble acid properties; for example, Cl_2O and N_2O.

R_2O_2 or RO; salts RX_2, hydroxides $R(OH)_2$. The most simple basic salts R_2OX_2 or R(OH)X; for instance, the chloride Zn_2OCl_2; also an almost exclusively basic type; but the basic properties are more feebly developed than in the preceding type. For example, CaO, MgO, BaO, PbO, FeO, MnO, &c.

R_2O_3; salts RX_3, hydroxides $R(OH)_3$, RO(OH), the most simple basic salts ROX, $R(OH)X_3$. The bases are feeble, like Al_2O_3, Fe_2O_3, Tl_2O_3, Sb_2O_3. The acid properties are also feebly developed; for instance, in B_2O_3; but with the non-metals the properties of acids are already clear; for instance, P_2O_3, $P(OH)_3$.

R_2O_4 or RO_2; salts RX_4 or ROX_2, hydroxides $R(OH)_4$, $RO(OH)_2$. Rarely bases (feeble), like ZrO_2, PtO_2, more often acid oxides; but the acid properties are in general feeble, as in CO_2, SO_2, SnO_2. Many intermediate oxides appear in this and the preceding and following types.

R_2O_5; salts principally of the types ROX_3, RO_2X, $RO(OH)_3$, $RO_2(OH)$, rarely RX_5. The basic character (X, a halogen, simple or complex; for instance, NO_3, Cl, &c.) is feeble, the acid character predominates, as is seen in N_2O_5, P_2O_5, Cl_2O_5, then X=OH, OK, &c., for example, $NO_2(OK)$.

R_2O_6 or RO_3; salts and hydroxides generally of the type RO_2X_2, $RO_2(OH)_2$. Oxides of an acid character, as SO_3, CrO_3, MnO_3. Basic properties rare and feebly developed as in UO_3.

R_2O_7; salts of the form RO_3X, $RO_3(OH)$, acid oxides; for instance, Cl_2O_7, Mn_2O_7. Basic properties as feebly developed as the acid properties in the oxides R_2O.

R_2O_8 or RO_4. A very rare type, and only known in OsO_4 and RuO_4.

It is evident from the circumstance that in all the higher types the *acid hydroxides* (for example, $HClO_4$, H_2SO_4, H_3PO_4) and salts with a single atom of one element contain, like the higher saline type RO_4, *not more than four atoms of oxygen;* that the formation of the saline oxides is governed by a certain common principle which is best looked for in the fundamental properties of oxygen, and in general of the most simple compounds. The hydrate of the oxide RO_2 is of the higher type $RO_22H_2O = RH_4O_4 = R(HO)_4$. Such, for example, is the hydrate of silica and the salts

(orthosilicates) corresponding with it, $Si(MO)_4$. The oxide R_2O_5 corresponds with the hydrate $R_2O_53H_2O = 2RH_3O_4 = 2RO(OH)_3$. Such is orthophosphoric acid, PH_3O_3. The hydrate of the oxide RO_3 is $RO_3H_2O = RH_2O_4 = RO_2(OH)_2$—for instance, sulphuric acid. The hydrate corresponding to R_2O_7 is evidently $RHO = RO_3(OH)$—for example, perchloric acid. Here, besides containing O_4, it must further be remarked that *the amount of hydrogen in the hydrate is equal to the amount of hydrogen in the hydrogen compound.* Thus silicon gives SiH_4 and SiH_4O_4, phosphorus PH_3 and PH_3O_4, sulphur SH_2 and SH_2O_4, chlorine ClH and $ClHO_4$. This, if it does not explain, at least connects in a harmonious and general system the fact that *the elements are capable of combining with a greater amount of oxygen, the less the amount of hydrogen which they are able to retain.* In this the key to the comprehension of all further deductions must be looked for, and we will therefore formulate this rule in general terms. An element R gives a hydrogen compound RH_n, the hydrate of its higher oxide will be RH_nO_4, and therefore the higher oxide will contain $2RH_nO_4—nH_2O = R_2O_{8-n}$. For example, chlorine gives ClH, hydrate $ClHO_4$, and the higher oxide Cl_2O_7. Carbon gives CH_4 and CO_2. So also, SiO_2 and SiH_4 are the higher compounds of silicon with hydrogen and oxygen, like CO_2 and CH_4. Here the amounts of oxygen and hydrogen are equivalent. Nitrogen combines with a large amount of oxygen, forming N_2O_5, but, on the other hand, with a small quantity of hydrogen in NH_3. *The sum of the equivalents of hydrogen and oxygen,* occurring in combination with an atom of nitrogen, is, as always in the higher types, equal to *eight.* It is the same with the other elements which combine with hydrogen and oxygen. Thus sulphur gives SO_3; consequently, six equivalents of oxygen fall to an atom of sulphur, and in SH_2 two equivalents of hydrogen. The sum is again equal to eight. The relation between Cl_2O_7 and ClH is the same. This shows that the property of elements of combining with such different elements as oxygen and hydrogen is subject to one common law, which is also formulated in the system of the elements presently to be described.[1]

[1]The hydrogen compounds, R_2H, in equivalency correspond with the type of the suboxides, R_4O. Palladium, sodium, and potassium give such hydrogen compounds, and it is worthy of remark that according to the periodic system these elements stand near to each other, and that in those groups where the hydrogen compounds R_2H appear, the quaternary oxides R_4O are also present.

Not wishing to complicate the explanation, I here only touch on the general features of the relation between the hydrates and oxides and of the oxides among themselves. Thus, for instance, the conception of the ortho-acids and of the normal acids will be considered in speaking of phosphoric and phosphorous acids.

As in the further explanation of the periodic law only those oxides which give salts will be considered, I think it will not be superfluous to mention here the following facts relative to the peroxides. Of the *peroxides* corresponding with hydrogen peroxide, the following are at present known: H_2O_2, Na_2O_2, S_2O_7 (as HSO_4?), K_2O_4, K_2O_2, CaO_2, TiO_3, Cr_2O_7, CuO_2(?), ZnO_2, Rb_2O_2, SrO_2, Ag_2O_2, CdO_2, CsO_2, Cs_2O_2, BaO_2, Mo_2O_7, SnO_3, W_2O_7, UO_4. It is probable that the number of peroxides will increase with further investigation. A periodicity is seen in those now known, for the elements (excepting

In the preceding we see not only the regularity and simplicity which govern the formation and properties of the oxides and of all the compounds of the elements, but also a fresh and exact means for recognising the analogy of elements. Analogous elements give compounds of analogous types, both higher and lower. If CO_2 and SO_2 are two gases which closely resemble each other both in their physical and chemical properties, the reason of this must be looked for not in an analogy of sulphur and carbon, but in that identity of the type of combination, RX_4, which both oxides assume, and in that influence which a large mass of oxygen always exerts on the properties of its compounds. In fact, there is little resemblance between carbon and sulphur, as is seen not only from the fact that CO_2 is the *higher form* of oxidation, whilst SO_2 is able to further oxidise into SO_3, but also from the fact that all the other compounds—for example, SH_2 and CH_4, SCl_2 and CCl_4, &c.—are entirely unlike both in type and in chemical properties. This absence of analogy in carbon and sulphur is especially clearly seen in the fact that the highest saline oxides are of different composition, CO_2 for carbon, and SO_3 for sulphur. Previously we considered the limit to which carbon tends in its compounds, and in a similar manner there is for every element in its compounds a tendency to attain a certain highest limit RX_n. This view was particularly developed in the middle of the present century by Frankland in studying the metallo-organic compounds, *i.e.* those in which X is wholly or partially a hydrocarbon radicle; for instance, X=CH_3 or C_2H_5 &c. Thus, for example, antimony, Sb, gives, with chlorine, compounds SbCl and $SbCl_5$ and corresponding oxygen compounds Sb_2O_3 and Sb_2O_5, whilst under the action of CH_3I, C_2H_3I, or in general EI (where E is a hydrocarbon radicle of the paraffin series), upon antimony or its alloy with sodium there are formed SbE_3 (for example, $Sb(CH_3)_3$, boiling at about 81°), which, corresponding to the lower form of combination SbX_3, are able to combine further with EI, or Cl_2, or O, and to form compounds of the limiting type SbX_5; for example, SbE_4Cl corresponding to NH_4Cl with the substitution of nitrogen by antimony, and of hydrogen by the hydrocarbon radicle. The elements which are most chemically analogous are characterised by the fact of their giving compounds of similar form RX_n. The halogens which are analogous give both higher and lower compounds. So also do

Li) of the first group, which give R_2O, form peroxides, and then the elements of the sixth group seem also to be particularly inclined to form peroxides, R_2O_7; but at present it is too early, in my opinion, to enter upon a generalisation of this subject, not only because it is a new and but little studied matter (not investigated for all the elements), but also, and more especially, because in many instances only the hydrates are known—for instance, $Mo_2H_2O_8$—and they perhaps are only compounds of peroxide of hydrogen —for example, $Mo_2H_2O_8 = 2MoO_3 + H_2O_2$—since Professor Schöne has shown that H_2O_2 and BaO_2 possess the property of combining together and with other oxides. Nevertheless, I have, in the general table expressing the periodic properties of the elements, endeavoured to sum up the data respecting all the known peroxide compounds whose characteristic property is seen in their capability to form peroxide of hydrogen under many circumstances.

the metals of the alkalis and of the alkaline earths. And we saw that this analogy extends to the composition and properties of the nitrogen and hydrogen compounds of these metals, which is best seen in the salts. Many such groups of analogous elements have long been known. Thus there are analogues of oxygen, nitrogen, and carbon, and we shall meet with many such groups. But an acquaintance with them inevitably leads to the questions, what is the cause of analogy and what is the relation of one group to another? If these questions remain unanswered, it is easy to fall into error in the formation of the groups, because the notions of the degree of analogy will always be relative, and will not present any accuracy or distinctness. Thus lithium is analogous in some respects to potassium and in others to magnesium; beryllium is analogous to both aluminium and magnesium. Thallium has much kinship with lead and mercury, but some of its properties appertain to lithium and potassium. Naturally, where it is impossible to make measurements one is reluctantly obliged to limit oneself to approximate comparisons, founded on apparent signs which are not distinct and are wanting in exactitude. But in the elements there is one accurately measurable property, which is subject to no doubt —namely, that property which is expressed in their atomic weights. Its magnitude indicates the relative mass of the atom, or, if we avoid the conception of the atom, its magnitude shows the relation between the masses forming the chemical and independent individuals or elements. And according to the teaching of all exact data about the phenomena of nature, the mass of a substance is that property on which all its remaining properties must be dependent, because they are all determined by similar conditions or by those forces which act in the weight of a substance, and this is directly proportional to its mass. Therefore it is most natural to seek for a dependence between the properties and analogies of the elements on the one hand and their atomic weights on the other.

This is the fundamental idea which leads *to arranging all the elements according to their atomic weights.* A periodic repetition of properties is then immediately observed in the elements. We are already familiar with examples of this:—

F =19,	Cl =35.5,	Br =80,	I =127,
Na=23,	K =39,	Rb=85,	Cs=133,
Mg=24,	Ca=40,	Sr =87,	Ba=137.

The essence of the matter is seen in these groups. The halogens have smaller atomic weights than the alkali metals, and the latter than the metals of the alkaline earths. Therefore, *if all the elements be arranged in the order of their atomic weights, a periodic repetition of properties is obtained.* This is expressed by the *law of periodicity; the properties of the elements, as well as the forms and properties of their compounds, are in periodic dependence or (expressing ourselves algebraically) form a periodic function of the atomic weights of the elements.*[2] Table I of *the*

[2]In laying out the accumulated information respecting the elements, I had occasion to reflect on their mutual relations. At the beginning of 1869 I distributed among many

periodic system of the elements is designed to illustrate this law. It is arranged in conformity with the eight types of oxides described in the preceding pages, and those elements which give the oxides, R_2O and consequently salts RX, form the 1st group; the elements giving R_2O_2 or RO as their highest grade of oxidation belong to the 2nd group, those giving R_2O_3 as their highest oxides form the 3rd group, and so on; whilst the elements of all the groups which are nearest in their atomic weights are arranged in series from 1 to 12. The even and uneven series of the same groups present the same forms and limits, but differ in their properties, and therefore two contiguous series, one even and the other uneven—for instance, the 4th and 5th—form a period. Hence the elements of the 4th, 6th, 8th, 10th, and 12th, or of the 3rd, 5th, 7th, 9th, and 11th,

chemists a pamphlet entitled 'An Attempted System of the Elements, based on their Atomic Weights and Chemical Analogies,' and at the March meeting of the Russian Chemical Society, 1869, I communicated a paper 'On the Correlation of the Properties and Atomic Weights of the Elements.' The substance of this paper is embraced in the following conclusions: (1) The elements, if arranged according to their atomic weights, exhibit an evident *periodicity* of properties. (2) Elements which are similar as regards their chemical properties have atomic weights which are either of nearly the same value (platinum, iridium, osmium) or which increase regularly (*e.g.* potassium, rubidium, cæsium). (3) The arrangement of the elements or of groups of elements in the order of their atomic weights corresponds with their so-called *valencies*. (4) The elements, which are the most widely distributed in nature, have *small* atomic weights, and all the elements of small atomic weight are characterised by sharply defined properties. They are therefore typical elements. (5) The *magnitude* of the atomic weight determines the character of an element. (6) The discovery of many yet unknown elements may be expected. For instance, elements analogous to aluminium and silicon, whose atomic weights would be between 65 and 75. (7) The atomic weight of an element may sometimes be corrected by aid of a knowledge of those of the adjacent elements. Thus the combining weight of tellurium must lie between 123 and 126, and cannot be 128. (8) Certain characteristic properties of the elements can be foretold from their atomic weights.

The entire periodic law is included in these lines. In the series of subsequent papers (1870–72, for example, in the *Transactions* of the Russian Chemical Society, of the Moscow Meeting of Naturalists, of the St. Petersburg Academy, and Liebig's *Annalen*) on the same subject we only find applications of the same principles, which were afterwards confirmed by the labours of Roscoe, Carnelley, Thorpe, and others in England; of Rammelsberg (cerium and uranium), L. Meyer (the specific volumes of the elements), Zimmermann (uranium), and more especially of C. Winkler (who discovered germanium, and showed its identity with ekasilicon), and others in Germany; of Lecoq de Boisbaudran in France (the discoverer of gallium = ekaaluminium); of Clève (the atomic weights of the cerium metals), Nilson (discoverer of scandium = ekaboron), and Nilson and Pettersson (determination of the vapour density of beryllium chloride) in Sweden; and of Brauner (who investigated cerium, and determined the combining weight of tellurium = 125) in Austria, and Piccini in Italy.

I consider it necessary to state that, in arranging the periodic system of the elements, I made use of the previous researches of Dumas, Gladstone, Pettenkofer, Kremers, and Lenssen on the atomic weights of related elements, but I was not acquainted with the works preceding mine of De Chancourtois (*vis tellurique*, or the spiral of the elements according to their properties and equivalents) in France, and of J. Newlands

series form analogues, like the halogens, the alkali metals, &c. The conjunction of two series, one even and one contiguous uneven series, thus forms one large *period*. These periods, beginning with the alkali metals, end with the halogens. The elements of the first two series have the lowest atomic weights, and in consequence of this very circumstance, although they bear the general properties of a group, they still show many peculiar and independent properties. Thus fluorine, as we know, differs in many points from the other halogens, and lithium from the other alkali metals, and so on. These lightest elements may be termed *typical elements*. They include—

H.
Li, Be, B, C, N, O, F.
Na, Mg . . . . .

In the annexed table all the remaining elements are arranged, not in groups and series, but *according to periods*. In order to understand the essence of the matter, it must be remembered that here the atomic weight gradually increases along a given line; for instance, in the line commencing with K=39 and ending with Br=80, the intermediate elements have intermediate atomic weights, as is clearly seen in Table II, where the elements stand in the order of their atomic weights.

I	II	III	IV	V	VI	VII					I	II	III	IV	V	VI	VII
		Even Series.															
												Mg	Al	Si	P	S	Cl
K	Ca	Sc	Ti	V	Cr	Mn	Fe	Co	Ni	Cu		Zn	Ga	Ge	As	Se	Br
Rb	Sr	Y	Zr	Nb	Mo	—	Ru	Rh	Pd	Ag		Cd	In	Sn	Sb	Te	I
Cs	Ba	La	Ce	Di ?	—	—	—	—	—	—		—	—	—	—	—	—
—	—	Yb	—	Ta	W	—	Os	Ir	Pt	Au		Hg	Tl	Pb	Bi	—	—
—	—	—	Th	—	U												
												Uneven Series.					

The same degree of analogy that we know to exist between potassim, rubidium, and cæsium; or chlorine, bromine, and iodine; or calcium,

(Law of Octaves—for instance, H, F, Cl, Co, Br, Pd, I, Pt form the first octave, and O, S, Fe, Se, Rh, Te, Au, Th the last) in England, although certain germs of the periodic law are to be seen in these works. With regard to the work of Professor Lothar Meyer respecting the periodic law (Notes 5 and 6), it is evident, judging from the method of investigation, and from his statement (Liebig's *Annalen, Supt. Band* 7, 1870, 354), at the very commencement of which he cites my paper of 1869 above mentioned, that he accepted the periodic law in the form which I proposed.

In concluding this historical statement I consider it well to observe that no law of nature, however general, has been established all at once; its recognition is always preceded by many hints; the establishment of a law, however, does not take place when its significance is recognised, but only when it has been confirmed by experiment, which the man of science must consider as the only proof of the correctness of his conjecture and opinions. I therefore, for my part, look upon Roscoe, De Boisbaudran, Nilson, Winkler, Brauner, Carnelley, Thorpe, and others who verified the adaptability of the periodic law to chemical facts, as the true founders of the periodic law, the further development of which still awaits fresh workers.

strontium, and barium, also exists between the elements of the other vertical columns. Thus, for example, zinc, cadmium, and mercury present a very close analogy with magnesium. For a true comprehension of the matter[3] it is very important to see that all the aspects of the distribution

[3]Besides arranging the elements (*a*) in a successive order according to their atomic weights, with indication of their analogies by showing some of the properties—for instance, their power of giving one or another form of combination—both of the *elements* and of their compounds (as is done in Table II), (*b*) according to periods (as in Table I), and (*c*) according to groups and series or small periods (as in the same tables), I am acquainted with the following methods of expressing the periodic relations of the elements: (1) By a curve drawn through points obtained in the following manner: The elements are arranged along the horizontal axis as abscissæ at distances from zero proportional to their atomic weights, whilst the values for all the elements of some property—for example, the specific volumes or the melting points, are expressed by the ordinates. This method, although graphic, has the theoretical disadvantage that it does not in any way indicate the existence of a limited and definite number of elements in each period. There is nothing, for instance, in this method of expressing the law of periodicity to show that between magnesium and aluminium there can be no other element with an atomic weight of, say, 25, atomic volume 13, and in general having properties intermediate between those of these two elements. The actual periodic law does not correspond with a continuous change of properties, with a continuous variation of atomic weight—in a word, it does not express an uninterrupted function—and as the law is purely chemical, starting from the conception of atoms and molecules which combine in multiple proportions, with intervals (not continuously), it *above all* depends on there being but few types of compounds, which are arithmetically simple, *repeat themselves,* and offer no uninterrupted transitions, so that each period can only contain a definite number of members. For this reason there can be no other elements between magnesium, which gives the chloride $MgCl_2$, and aluminium, which forms AlX_3; there is a break in the continuity, according to the law of multiple proportions. The periodic law ought not, therefore, to be expressed by geometrical figures in which continuity is always understood. Owing to these considerations I never have and never will express the periodic relations of the elements by any geometrical figures. (2) *By a plane spiral.* Radii are traced from a centre, proportional to the atomic weights; analogous elements lie along one radius, and the points of intersection are arranged in a spiral. This method, adopted by De Chancourtois, Baumgauer, E. Huth, and others, has many of the imperfections of the preceding, although it removes the indefiniteness as to the number of elements in a period. It is merely an attempt to reduce the complex relations to a simple graphic representation, since the equation to the spiral and the number of radii are not dependent upon anything. (3) *By the lines of atomicity,* either parallel, as in Reynolds's and the Rev. S. Haughton's method, or as in Crookes's method, arranged to the right and left of an axis, along which the magnitudes of the atomic weights are counted, and the position of the elements marked off, on the one side the members of the even series (paramagnetic, like oxygen, potassium, iron), and on the other side the members of the uneven series (diamagnetic, like sulphur, chlorine, zinc, and mercury). On joining up these points a periodic curve is obtained, compared by Crookes to the oscillations of a pendulum, and, according to Haughton, representing a cubical curve. This method would be very graphic did it not require, for instance, that sulphur should be considered as bivalent and manganese as univalent, although neither of these elements gives stable derivatives of these natures, and although the one is taken on the basis of the lowest possible compound SX_2, and the other of the highest, because manganese can be referred to the univalent elements only by the analogy of $KMnO_4$ to

of the elements according to their atomic weights essentially express one and the same fundamental *dependence—periodic properties.*[4] The following points then must be remarked in it.

$KClO_4$. Furthermore, Reynolds and Crookes place hydrogen, iron, nickel, cobalt, and others outside the axis of atomicity, and consider uranium as bivalent without the least foundation. (4) Rantsheff endeavoured to classify the elements in their periodic relations by a system dependent on solid geometry. He communicated this mode of expression to the Russian Chemical Society, but his communication, which is apparently not void of interest, has not yet appeared in print. (5) *By algebraic formulæ:* for example, E. J. Mills (1886) endeavours to express all the atomic weights by the logarithmic function $A=15\ (n-0{\cdot}9375t)$, in which the variables n and t are whole numbers. For instance, for oxygen $n=2$, $t=1$; hence $A=15{\cdot}94$; for antimony $n=9$, $t=0$; whence $A=120$, and so on, n varies from 1 to 16 and t from 0 to 59. The analogues are hardly distinguishable by this method: thus for chlorine the magnitudes of n and t are 3 and 7; for bromine 6 and 6; for iodine 9 and 9; for potassium 3 and 14; for rubidium 6 and 18; for cæsium 9 and 20; but a certain regularity seems to be shown. (6) A more natural method of expressing the dependence of the properties of elements on their atomic weights is obtained by *trigonometrical functions,* because this dependence is periodic like the functions of trigonometrical lines, and therefore Ridberg in Sweden (Lund, 1885) and F. Flavitzky in Russia (Kazan, 1887) have adopted a similar method of expression, which must be considered as worthy of being worked out, although it does not express the absence of intermediate elements—for instance, between magnesium and aluminium, which is essentially the most important part of the matter. (7) The investigations of B. N. Tchitchérin (1888, *Journal of the Russian Physical and Chemical Society*) form the first effort in the latter direction. He carefully studied the alkali metals, and discovered the following simple relation between their atomic volumes: they can all be expressed by $A(2-0{\cdot}0428\ An)$, where A is the atomic weight and $n=1$ for lithium and sodium, $\frac{4}{8}$ for potassium, $\frac{3}{8}$ for rubidium, and $\frac{2}{8}$ for cæsium. If n always $= 1$, then the volume of the atom would become zero at $A=46\frac{2}{3}$, and would reach its maximum when $A=23\frac{1}{3}$, and the density increases with the growth of A. In order to explain the variation of n, and the relation of the atomic weights of the alkali metals to those of the other elements, as also the atomicity itself, Tchitchérin supposes all atoms to be built up of a primary matter; he considers the relation of the central to the peripheric mass, and, guided by mechanical principles, deduces many of the properties of the atoms from the reaction of the internal and peripheric parts of each atom. This endeavour offers many interesting points, but it admits the hypothesis of the building up of all the elements from one primary matter, and at the present time such an hypothesis has not the least support either in theory or in fact.

[4]Many natural phenomena exhibit a dependence of a periodic character. Thus the phenomena of day and night and of the seasons of the year, and vibrations of all kinds, exhibit variations of a periodic character in dependence on time and space. But in ordinary periodic functions one variable varies continuously, whilst the other increases to a limit, then a period of decrease begins, and having in turn reached its limit a period of increase again begins. It is otherwise in the periodic function of the elements. Here the mass of the elements does not increase continuously, but abruptly, by steps, as from magnesium to aluminium. So also the valency or atomicity leaps directly from 1 to 2 to 3, &c., without intermediate quantities, and in my opinion it is these properties which are the most important, and it is their periodicity which forms the substance of the periodic law. It expresses *the properties of the real elements,* and not of what may be termed their manifestations visually known to us. The external properties of

1. The composition of the higher oxygen compounds is determined by the groups: the first group gives R_2O, the second R_2O_2 or RO, the third R_2O_3, &c. There are eight type of oxides and therefore eight groups. Two series give a period, and the same type of oxide is met with twice in a period. For example, in the period beginning with potassium, oxides of the composition RO are formed by calcium and zinc, and of the composition RO_3 by molybdenum and tellurium. The oxides of the even series, of the same type, have stronger basic properties than the oxides of the uneven series, and the latter as a rule are endowed with an acid character. Therefore the elements which exclusively give bases, like the alkali metals, will be found at the commencement of the period, whilst such purely acid elements as the halogens will be at the end of the period. The interval will be occupied by intermediate elements. It must be observed that the acid character is chiefly proper to the elements with small atomic weights in the uneven series, whilst the basic character is exhibited by the heavier elements in the even series. Hence elements which give acids chiefly predominate among the lightest (typical) elements, especially in the last groups; whilst the heaviest elements, even in the last groups (for instance, thallium, uranium), have a basic character. Thus the basic and acid characters of the higher oxides are determined (*a*) by the type of oxide, (*b*) by the even or uneven series, and (*c*) by the atomic weight. The groups are indicated by Roman numerals from I to VIII.

2. *The hydrogen compounds* being volatile or gaseous substances which are prone to reaction—such as HCl, H_2O, H_3N, and H_4C—are only formed by the elements of the uneven series and higher groups giving oxides of the forms R_2O_n, RO_3, R_2O_5, and RO_2.

3. If an element gives a hydrogen compound, RX_m, it forms an *organo-metallic compound* of the same composition, where $X=C_nH_{2n+1}$; that is, X is the radicle of a saturated hydrocarbon. The elements of the uneven series, which are incapable of giving hydrogen compounds, and give oxides of the forms RX, RX_2, RX_3, also give organo-metallic compounds of this form proper to the higher oxides. Thus zinc forms the oxide ZnO, salts ZnX_2, and zinc ethyl $Zn(C_2H_5)_2$. The elements of the even series do not seem to form organo-metallic compounds at all; at

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elements and compounds are in periodic dependence on the atomic weight of the elements only because these external properties are themselves the result of the properties of the real elements which unite to form the "free" elements and the compounds. To explain and express the periodic law is to explain and express the cause of the law of multiple proportions, of the difference of the elements, and the variation of their atomicity, and at the same time to understand what mass and gravitation are. In my opinion this is still premature. But just as without knowing the cause of gravitation, it is possible to make use of the law of gravity, so for the aims of chemistry it is possible to take advantage of the laws discovered by chemistry without being able to explain their causes. The above-mentioned peculiarity of the laws of chemistry respecting definite compounds and the atomic weights leads one to think that the time has not yet come for their full explanation, and I do not think that it will come before the explanation of such a primary law of nature as the law of gravity.

least all efforts for their preparation have as yet been fruitless—for instance, in the case of titanium, zirconium, or iron.

4. The atomic weights of elements belonging to contiguous periods differ approximately by 45; for example, K<Rb, Cr<Mo, Br<I. But the elements of the typical series show much smaller differences. Thus the difference between the atomic weights of Li, Na, and K, between Ca, Mg, and Be, between Si and C, between S and O, and between Cl and F, is 16. As a rule, there is a greater difference between the atomic weights of two elements of one group and belonging to two neighboring series (Ti—Si =V—P=Cr—S=Mn—Cl=Nb—As, &c.=20); and this difference attains a maximum with the heaviest elements (for example, Th—Pb=26, Bi—Ta =26, Ba—Cd=25, &c.). Furthermore, the difference between the atomic weights of the elements of even and uneven series also increases. In fact, the differences between Na and K, Mg and Ca, Si and Ti, are less abrupt than those between Pb and Th, Ta and Bi, Cd and Ba, &c. Thus even in the magnitude of the differences of the atomic weights of analogous elements there is observable a certain connection with the gradation of their properties.[5]

5. According to the periodic system every element occupies a certain position, determined by the group (indicated in Roman numerals) and series (Arabic numerals) in which it occurs. These indicate the atomic weight, the analogues, properties, and type of the higher oxide, and of the hydrogen and other compounds—in a word, all the chief quantitative and qualitative features of an element, although there yet remains a whole series of further details and peculiarities whose cause should perhaps be looked for in small differences of the atomic weights. If in a certain group there occur elements, R, R_2, R_3, and if in that series which contains one of these elements, for instance R_2, an element Q_2 precedes it and an element T_2 succeeds it, then the properties of R_2 are determined by the properties of R_1, R_3, Q_2, and T_2. Thus, for instance, the atomic weight of $R_2 = \frac{1}{4}(R_1+R_3+Q_2+T_2)$. For example, selenium occurs in the same group as sulphur, S = 32, and tellurium, Te = 125, and, in the

[5]The relation between the atomic weights, and especially the difference=16, was observed in the sixth and seventh decades of this century by Dumas, Pettenkofer, L. Meyer, and others. Thus Lothar Meyer in 1864, following Dumas and others, grouped together the tetravalent elements carbon and silicon; the trivalent elements nitrogen, phosphorus, arsenic, antimony, and bismuth; the bivalent oxygen, sulphur, selenium, and tellurium; the univalent fluorine, chlorine, bromine, and iodine; the univalent metals lithium, sodium, potassium, rubidium, cæsium, and thallium, and the bivalent metals beryllium, magnesium, strontium and barium—observing that in the first the difference is, in general=16, in the second about=46, and the last about=87–90. The first germs of the periodic law are visible in such observations as these. Since its establishment this subject has been most fully worked out by Ridberg (Note 3), who observed a periodicity in the variation of the differences between the atomic weights of two contiguous elements, and its relation to their atomicity. A. Bazaroff (1887) investigated the same subject, taking, not the arithmetical differences of contiguous and analogous elements, but the ratio of their atomic weights; and he also observed that this ratio alternately rises and falls with the rise of the atomic weights.

7th series As = 75 stands before it and Br = 80 after it. Hence the atomic weight of selenium should be $\frac{1}{4}(32+125+75+80) = 78$, which is near to the truth. Other properties of selenium may also be determined in this manner. For example, arsenic forms H_3As, bromine gives HBr, and it is evident that selenium, which stands between them, should form H_2Se, with properties intermediate between those of H_3As and HBr. Even the physical properties of selenium and its compounds, not to speak of their composition, being determined by the group in which it occurs, may be foreseen with a close approach to reality from the properties of sulphur, tellurium, arsenic, and bromine. *In this manner it is possible to foretell the properties of still unknown elements.* For instance, in the position IV, 5—that is, in the IVth group and 5th series—an element is still wanting. These unknown elements may be named after the preceding known element of the same group by adding to the first syllable the prefix *eka-*, which means *one* in Sanskrit. The element IV, 5, follows after IV, 3, and this latter position being occupied by silicon, we call the unknown element ekasilicon and its symbol Es. The following are the properties which this element should have on the basis of the known properties of silicon, tin, zinc, and arsenic. Its atomic weight is nearly 72, higher oxide EsO_2, lower oxide EsO, compounds of the general form EsX_4, and chemically unstable lower compounds of the form EsX_2. Es gives volatile organo-metallic compounds—for instance, $Es(CH_3)_4$, $Es(CH_3)_3Cl$, and $Es(C_2H_5)_4$, which boil at about 160°, &c.; also a volatile and liquid chloride, $EsCl_4$, boiling at about 90° and of specific gravity about 1·9. EsO_2 will be the anhydride of a feeble colloidal acid, metallic Es will be rather easily obtainable from the oxides and from K_2EsF_6 by reduction, EsS_2 will resemble SnS_2 and SiS_2, and will probably be soluble in ammonium sulphide; the specific gravity of Es will be about 5·5, EsO_2 will have a density of about 4·7, &c. Such a prediction of the properties of ekasilicon was made by me in 1871, on the basis of the properties of the elements analogous to it: IV, 3, = Si, IV, 7 = Sn, and also II, 5 = Zn and V, 5 = As. And now that this element has been discovered by C. Winkler, of Freiberg, it has been found that its actual properties entirely correspond with those which were foretold.[6] In this we see a most impor-

[6]The laws of nature admit of no exceptions, and in this they clearly differ from such rules and maxims as are found in grammar, and other inventions, methods, and relations of man's creation. The confirmation of a law is only possible by deducing consequences from it, such as could not possibly be foreseen without it, and by verifying those consequences by experiment and further proofs. Therefore, when I conceived the periodic law, I (1869–1871) deduced such logical consequences from it as could serve to show whether it were true or not. Among them was the prediction of the properties of undiscovered elements and the correction of the atomic weights of many, and at that time little known, elements. Thus uranium was considered as trivalent, U=120; but as such it did not correspond with the periodic law. I therefore proposed to double its atomic weight—U=240, and the researches of Roscoe, Zimmermann, and others justified this alteration. It was the same with cerium, whose atomic weight it was necessary to change according to the periodic law. I therefore determined its specific heat, and the result I obtained was verified by the new determinations of

tant confirmation of the truth of the periodic law. This element is now called germanium, Ge. It is not the only one that has been predicted by the periodic law.[7] Properties were foretold of an element ekaaluminium, III, 5, El = 68, and were afterwards verified when the metal termed "gallium" was discovered by De Boisbaudran. So also the properties of scandium corresponded with those predicted for ekaboron, according to Nilson.

6. As a true law of nature is one to which there are no exceptions, the periodic dependence of the properties on the atomic weights of the elements gives a *new means for determining by the equivalent the atomic weight* or atomicity of imperfectly investigated but known elements, for which no other means could as yet be applied for determining the true atomic weight. At the time (1869) when the periodic law was first proposed there were several such elements. It thus became possible to learn their true atomic weights, and these were verified by later researches. Among the elements thus concerned were indium, uranium, cerium, yttrium, and others.[8]

Hillebrand. I then corrected certain formulæ of the cerium compounds, and the researches of Rammelsberg, Brauner, Cleve, and others verified the proposed alteration. It was necessary to do one or the other—either to consider the periodic law as completely true, and as forming a new instrument in chemical research, or to refute it. Acknowledging the method of experiment to be the only true one, I myself verified what I could, and gave everyone the possibility of proving or confirming the law, and did not think, like L. Meyer (Liebig's *Annalen, Supt. Band* 7, 1870, 364), when writing about the periodic law that "it would be rash to change the accepted atomic weights on the basis of so uncertain a starting point." In my opinion, the basis offered by the periodic law had to be verified or refuted, and experiment in every case verified it. The starting point then became general. No law of nature can be established without such a method of testing it. Neither De Chancourtois, to whom the French ascribe the discovery of the periodic law, nor Newlands, who is put forward by the English, nor L. Meyer, who is now cited by many as its founder, ventured to foretell the *properties* of undiscovered elements, or to alter the "accepted atomic weights," or, in general, to regard the periodic law as a new, strictly established law of nature, as I did from the very beginning (1869).

[7]When in 1871 I wrote a paper on the application of the periodic law to the determination of the properties of hitherto undiscovered elements, I did not think I should live to see the verification of this consequence of the law, but such was to be the case. Three elements were described—ekaboron, ekaaluminium, and ekasilicon—and now, after the lapse of twenty years, I have had the great pleasure of seeing them discovered and named Gallium, Scandium, and Germanium, after those three countries where the rare minerals containing them are found, and where they were discovered. For my part I regard L. de Boisbaudran, Nilson, and Winkler, who discovered these elements, as the true corroborators of the periodic law. Without them it would not have been accepted to the extent it now is.

[8]Taking indium, which occurs together with zinc, as our example, we will show the principle of the method employed. The equivalent of indium to hydrogen in its oxide is 37·7—that is, if we suppose its composition to be like that of water; then In=37·7, and the oxide of indium is In_2O. The atomic weight of indium was taken as double the equivalent—that is, indium was considered to be a bivalent element—and

7. The periodic variability of the properties of the elements in dependence on their masses presents a distinction from other kinds of periodic dependence (as, for example, the sines of angles vary periodically and successively with the growth of the angles, or the temperature of the atmosphere with the course of time), in that the weights of the atoms do not increase gradually, but by leaps, that is, according to Dalton's law of multiple proportions, there not only are not, but there cannot be, any transitive or intermediate elements between two neighbouring ones (for example, between K = 39 and Ca = 40, or Al = 27 and Si = 28, or C = 12 and N = 14, &c.). As in a molecule of a hydrogen compound there may be either one, as in HF, or two, as in H_2O, or three, as in NH_3, &c., atoms of hydrogen; but as there cannot be molecules containing $2\frac{1}{2}$ atoms of hydrogen to one atom of another element, so there cannot be any element intermediate between N and O, with an atomic weight greater than 14 or less than 16, or between K and Ca. Hence the periodic dependence of the elements cannot be expressed by any algebraical continuous function in the same way that it is possible, for instance, to express the variation of the temperature during the course of a day or year.

8. The essence of the notions giving rise to the periodic law consists in a general physico-mechanical principle which recognises the correla-

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In=2×37·7=75·4. If indium only formed an oxide, RO, it should be placed in group II. But in this case it appears that there would be no place for indium in the system of the elements, because the positions II, 5 = Zn = 65 and II, 6 = Sr = 87 were already occupied by known elements, and according to the periodic law an element with an atomic weight 75 could not be bivalent. As neither the vapour density nor the specific heat, nor even the isomorphism (the salts of indium crystallise with great difficulty), of the compounds of indium were known, there was no reason for considering it to be a bivalent metal, and therefore it might be regarded as trivalent, quadrivalent, &c. If it be trivalent, then In=3×37·7=113, and the composition of the oxide is In_2O_3, and of its salts InX_3. In this case it at once falls into its place in the system, namely, in group III and 7th series, between Cd=112 and Sn=118, as an analogue of aluminium or dvialuminium (dvi = 2 in Sanskrit). All the properties observed in indium correspond with this position; for example, the density, cadmium = 8·6, indium = 7·4; tin = 7·2; the basic properties of the oxides CdO, In_2O_3, SnO_2, successively vary, so that the properties of In_2O_3 are intermediate between those of CdO and SnO_2 or Cd_2O_2 and Sn_2O_4. That indium belongs to group III has been confirmed by the determination of its specific heat, (0·057 according to Bunsen, and 0·055 according to me) and also by the fact that indium forms alums like aluminium, and therefore belongs to the same group.

The same kind of considerations necessitated taking the atomic weight of titanium as nearly 48, and not as 52, the figure derived from many analyses. And both these corrections, made on the basis of the law, have now been confirmed, for Thorpe found, by a series of careful experiments, the atomic weight of titanium to be that foreseen by the periodic law. Notwithstanding that previous analyses gave Os=199·7, Ir=198, and Pt=187, the periodic law shows, as I remarked in 1871, that the atomic weights should rise from osmium to platinum and gold, and not fall. Many recent researches, and especially those of Seubert, have fully verified this statement, based on the law. Thus a true law of nature anticipates facts, foretells magnitudes, gives a hold on nature, and leads to improvements in the methods of research, &c.

tion, transmutability, and equivalence of the forces of nature. Gravitation, attraction at small distances, and many other phenomena are in direct dependence on the mass of matter. It might therefore have been expected that chemical forces would also depend on mass. A dependence is in fact shown, the properties of elements and compounds being determined by the masses of the atoms of which they are formed. The weight of a molecule, or its mass, determines many of its properties independently of its composition. Thus carbonic oxide, CO, and nitrogen, N_2, are two gases having the same molecular weight, and many of their properties (density, liquefaction, specific heat, &c.) are similar or nearly similar. The differences dependent on the nature of a substance play another part, and form magnitudes of another order. But the properties of atoms are mainly determined by their mass or weight, and are in dependence upon it. Only in this case there is a peculiarity in the dependence of the properties on the mass, for this *dependence is determined by a periodic law.* As the mass increases the properties vary, at first successively and regularly, and then return to their original magnitude and recommence a fresh period of variation like the first. Nevertheless here as in other cases a small variation of the mass of the atom generally leads to a small variation of properties, and determines differences of a second order. The atomic weights of cobalt and nickel, of rhodium, ruthenium, and palladium, and of osmium, iridium, and platinum, are very close to each other, and their properties are also very much alike—the differences are not very perceptible. And if the properties of atoms are a function of their weight, many ideas which have more or less rooted themselves in chemistry must suffer change and be developed and worked out in the sense of this deduction. Although at first sight it appears that the chemical elements are perfectly independent and individual, instead of this idea of the nature of the elements, the notion of the dependence of their properties upon *their mass* must now be established; that is to say, the subjection of the individuality of the elements to a common higher principle which evinces itself in gravity and in all physico-chemical phenomena. Many chemical deductions then acquire a new sense and significance, and a regularity is observed where it would otherwise escape attention. This is more particularly apparent in the physical properties, to the consideration of which we shall afterwards turn, and we will now point out that Gustavson first, and subsequently Potilitzin, demonstrated the direct dependence of the reactive power on the atomic weight and that fundamental property which is expressed in the forms of their compounds, whilst in a number of other cases the purely chemical relations of elements proved to be in connection with their periodic properties. As a case in point, it may be mentioned that Carnelley remarked a dependence of the decomposability of the hydrates on the position of the elements in the periodic system; whilst L. Meyer, Willgerodt, and others established a connection between the atomic weight or the position of the elements in the periodic system and their property of serving as media in the transference of the halogens to the hydrocarbons. Bailey pointed out a periodicity in the sta-

bility (under the action of heat) of the oxides, namely: (*a*) in the even series (for instance, CrO_3, MoO_3, WO_3, and UO_3) the higher oxides of a given group decompose with greater ease the smaller the atomic weight, while in the uneven series (for example, CO_2, GeO_2, SnO_2, and PbO_2) the contrary is the case; and (*b*) the stability of the higher saline oxides in the even series (as in the fourth series from K_2O to Mn_2O_7) decreases in passing from the lower to the higher groups, while in the uneven series it increases from the Ist to the IVth group, and then falls from the IVth to VIIth; for instance, in the series Ag_2O, CdO, In_2O_3, SnO_2, and then SnO_2, Sb_2O_5, TeO_3, I_2O_7. K. Winkler looked for and actually found (1890) a dependence between the reducibility of the metals by magnesium and their position in the periodic system of the elements. The greater the attention paid to this field the more often is a distinct connection found between the variation of purely chemical properties of analogous substances and the variation of the atomic weights of the constituent elements and their position in the periodic system. Besides, since the periodic system has become more firmly established, many facts have been gathered, showing that there are many similarities between Sn and Pb, B and Al, Cd and Hg, &c., which had not been previously observed, although foreseen in some cases, and a consequence of the periodic law. Keeping our attention in the same direction, we see that the most widely distributed elements in nature are those with small atomic weights, whilst in organisms the lightest elements exclusively predominate (hydrogen, carbon, nitrogen, oxygen), whose small mass facilitates those transformations which are proper to organisms. Poluta (of Kharkoff), C. C. Botkin, Blake, Brenton, and others even discovered a correlation between the physiological action of salts and other reagents on organisms and the positions occupied in the periodic system by the metals contained in them.

As, from the necessity of the case, the physical properties must be in dependence on the composition of a substance, *i.e.* on the quality and quantity of the elements forming it, so for them also a dependence on the atomic weight of the component elements must be expected, and consequently also on their periodic distribution. We will content ourselves with citing the discovery by Carnelley in 1879 of the dependence of the magnetic properties of the elements on the position occupied by them in the periodic system. Carnelley showed that all the elements of the *even series* (beginning with lithium, potassium, rubidium, cæsium) belong to the number of magnetic (paramagnetic) substances; for example, according to Faraday and others, C, N, O, K, Ti, Cr, Mn, Fe, Co, Ni, Ce, are magnetic; and the elements of the *uneven series are diamagnetic,* H, Na, Si, P, S, Cl, Cu, Zn, As, Se, Br, Ag, Cd, Sn, Sb, I, Au, Hg, Tl, Pb, Bi.

Carnelley also showed that the *melting point* of elements varies periodically, as is seen by the figures in Table II (nineteenth column), where all the most trustworthy data are collected, and predominance is given to those having maximum and minimum values.

There is no doubt that many other physical properties will, when further studied, also prove to be in periodic dependence on the atomic weights, but at present only a few are known with any completeness, and we will only refer to the one which is the most easily and frequently determined—namely, the *specific gravity* in a solid and liquid state, the more especially as its connection with the chemical properties and relations of substances is shown at every step. Thus, for instance, of all the metals those of the alkalis, and of all the non-metals the halogens, are the most energetic in their reactions, and they have the lowest specific gravity among the adjacent elements, as is seen in Table II, column 17. Such are sodium, potassium, rubidium, cæsium among the metals, and chlorine, bromine, and iodine among the non-metals; and as such less energetic metals as iridium, platinum, and gold (and even charcoal or the diamond) have the highest specific gravity among the elements near to them in atomic weight; therefore the degree of the condensation of matter evidently influences the course of the transformations proper to a substance, and furthermore this dependence on the atomic weight, although very complex, is of a clearly periodic character. In order to account for this to some extent, it may be imagined that the lightest elements are porous, and, like a sponge, are easily penetrated by other substances, whilst the heavier elements are more compressed, and give way with difficulty to the insertion of other elements. These relations are best understood when, instead of the specific gravities referring to a unit of volume, the *atomic volumes of the elements*—that is, the quotient A/d of the atomic weight A by the specific gravity d—are taken for comparison. As, according to the entire sense of the atomic theory, the actual matter of a substance does not fill up its whole cubical contents, but is surrounded by a medium (ethereal, as is generally imagined), like the stars and planets which travel in the space of the heavens and fill it, with greater or less intervals, so the quotient A/d only expresses the *mean* volume corresponding to the sphere of the atoms, and therefore $\sqrt[3]{A/d}$ is *the mean distance between the centres of the atoms.* For compounds whose molecules weigh M, the mean magnitude of the atomic volume is obtained by dividing the mean molecular volume M/d by the number of atoms n in the molecule. The above relations may easily be expressed from this point of view by comparing the atomic volumes. Those comparatively light elements which easily and frequently enter into reaction have the greatest atomic volumes: sodium 23, potassium 45, rubidium 57, cæsium 71, and the halogens about 27; whilst with those elements which enter into reaction with difficulty, the mean atomic volume is small; for carbon in the form of a diamond it is less than 4, as charcoal about 6, for nickel and cobalt less than 7, for iridium and platinum about 9. The remaining elements having atomic weights and properties intermediate between those elements mentioned above have also intermediate atomic volumes. Therefore *the specific gravities and specific volumes of solids and liquids stand in periodic dependence on the atomic weights,* as is

seen in Table II, where both A (the atomic weight) and d (the specific gravity), and A/d (specific volumes of the atoms) are given (column 18).

Thus we find that in the large periods beginning with lithium, sodium, potassium, rubidium, cæsium, and ending with fluorine, chlorine, bromine, iodine, the extreme members (energetic elements) have a small density and large volume, whilst the intermediate substances gradually increase in density and decrease in volume—that is, as the atomic weight increases the density rises and falls, again rises and falls, and so on. Furthermore, the energy decreases as the density rises, and the greatest density is proper to the atomically heaviest and least energetic elements; for example, Os, Ir, Pt, Au, U.

In order to explain the relation between the volumes of the elements and of their compounds, the densities (column S) and volumes (column M/s) of some of the higher saline oxides arranged in the same order as in the case of the elements are given on p. 566. For convenience of comparison the volumes of the oxides are all calculated per two atoms of an element combined with oxygen. For example, the density of $Al_2O_3=4{\cdot}0$, weight $Al_2O_3=102$, volume $Al_2O_3=25{\cdot}5$. Whence, knowing the volume of aluminium to be 11, it is at once seen that in the formation of aluminium oxide, 22 volumes of it give 25·5 volumes of oxide. A distinct periodicity may also be observed with respect to the specific gravities and volumes of the higher saline oxides. Thus in each period, beginning with the alkali metals, the specific gravity of the oxides first rises, reaches a maximum, and then falls on passing to the acid oxides, and again becomes a minimum about the halogens. But it is especially important to call attention to the fact that the volume of the alkali oxides is less than that of the metal contained in them, which is also expressed in the last column, giving this difference for each atom of oxygen. Thus 2 atoms of sodium, or 46 volumes, give 24 volumes of Na_2O, and about 37 volumes of 2NaHO—that is, the oxygen and hydrogen in distributing themselves in the medium of sodium have not only not increased the distance between its atoms, but have brought them nearer together, have drawn them together by the force of their great affinity, by reason, it may be presumed, of the small mutual attraction of the atoms of sodium. Such metals as aluminium and zinc, in combining with oxygen and forming oxides of feeble salt-forming capacity, hardly vary in volume, but the common metals and non-metals, and especially those forming acid oxides, always give an increased volume when oxidised—that is, the atoms are set further apart in order to make room for the oxygen. The oxygen in them does not compress the molecule as in the alkalis; it is therefore comparatively easily disengaged.

As the volumes of the chlorides, organo-metallic and all other corresponding compounds, also vary in a like periodic succession with a change of elements, it is evidently possible to indicate the properties of substances yet uninvestigated by experimental means, and even those of yet undiscovered elements. It was possible by following this method to foretell, on the basis of the periodic law, many of the properties of scandium, gallium, and germanium, which were verified with great accuracy after

	S	M/s	Volume of Oxygen
H_2O	1·0	18	? — 22
Li_2O	2·0	15	— 9
Be_2O_2	3·06	16	+ 2·6
B_2O_3	1·8	39	+ 10·0
C_2O_4	1·6	55	+ 10·6
N_2O_5	1·64	66	? + 4
Na_2O	2·6	24	— 22
Mg_2O_2	3·5	23	— 4·5
Al_2O_3	4·0	26	+ 1·3
Si_2O_4	2·65	45	+ 5·2
P_2O_5	2·39	59	+ 6·2
S_2O_3	1·96	82	+ 8·7
Cl_2O_7	?1·92	95	+ 6
K_2O	2·7	35	— 35
Ca_2O_2	3·25	34	— 8
Sc_2O_3	3·86	35	? 0
Ti_2O_4	4·2	38	+ 3
V_2O_5	3·49	52	+ 6·7
Cr_2O_6	2·74	73	+ 9·5
Cu_2O	5·9	24	+ 9·6
Zn_2O_2	5·7	23	+ 4·8
Ga_2O_3	?5·1	36	+ 4
Ge_2O_4	4·7	44	+ 4·5
As_2O_5	4·1	56	+ 6·0
Sr_2O_2	4·7	44	— 13
Y_2O_3	5·0	45	? — 2
Zr_2O_4	5·5	44	0
Nb_2O_5	4·7	57	+ 6
MoO_3	4·4	65	+ 6·8
Ag_2O	7·5	31	+ 11
Cd_2O_2	8·0	32	+ 3
In_2O_3	7·18	38	+ 2·7
Sn_2O_4	7·0	43	+ 2·7
Sb_2O_5	6·5	49	+ 2·6
TeO_3	5·1	68	+ 4·7
Ba_2O_2	5·7	52	— 10
La_2O_3	6·5	50	+ 1
Ce_2O_4	6·74	50	+ 2
Ta_2O_5	7·5	59	+ 4·6
W_2O_6	6·8	68	+ 8·2
Hg_2O_2	11·1	39	+ 4·5
Pb_2O_4	8·9	53	+ 4·2
Th_2O_4	9·86	54	+ 2

these metals had been discovered. The periodic law, therefore, has not only embraced the mutual relations of the elements and expressed their analogy, but has also to a certain extent subjected to law the doctrine of the types of the compounds formed by the elements: it has enabled us to see a regularity in the variation of all chemical and physical properties of elements and compounds, and has rendered it possible to foretell the properties of elements and compounds yet uninvestigated by experimental means; thus it has prepared the ground for the building up of atomic and molecular mechanics.

RADIOACTIVITY

by

MARIE CURIE

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MARIE CURIE

1867–1934

MARIE CURIE was born Marie Sklodovska in Warsaw, in 1867, the youngest child of a poorly paid Polish teacher in the Russian-controlled schools of Warsaw. At three she had learned by herself to read; from an early age she displayed an infallible memory, quick comprehension, unbelievable powers of concentration. This precocity her parents, particularly her father, after his wife's death when Marie was scarcely more than an infant, tried to curb. But the four other children in the family also had great gifts, and the atmosphere of the household encouraged intellectual striving. At sixteen Marie had finished the course at the gymnasium and had there won the gold medal—the third to be carried off by the Sklodovski children. And after a year of visiting her relatives in rural Poland, she began earning her living as a private teacher in Warsaw.

Like the other young people of her set, she devoted herself to Comte, read Darwin and Pasteur, made an effort—patriotic in origin—to educate the illiterate poor, and joined secret classes for the study of science. The local university being open only to men, she and her favorite sister, Bronya, yearned to go to Paris to study. Finally she persuaded Bronya to take their slender resources and to proceed to the Sorbonne. Their plan was that once Bronya had qualified for her degree, she would aid Marie. Meantime, Marie would contribute what she could earn to Bronya's support.

There followed several years of service as a governess, now in Warsaw, now in a country village miles from a town. In the intervals of her exacting duties Marie found time to organize secret Polish classes for the children of the poor, to study mathematics, and to teach herself such chemistry and physics as she could dig out of textbooks without the aid of either teacher or laboratory. The dream of getting to Paris

faded. Then Bronya finished her medical course, married a fellow Pole in Paris, and began to practice. Suddenly Marie was summoned to her opportunity.

In 1889, almost without financial resources, Marie was living in Paris with her sister and was entered at the Sorbonne. Presently she felt that the gaiety of her sister's Polish friends—even the occasional concert and the occasional theater—interfered with her work. She moved to a lodging in the Latin Quarter. In that neighborhood, in one poor, unheated, almost barren room or another, she lived her student days. There, unable to cook, too poor to buy food and fuel, she studied early and late until she almost succumbed to overwork and malnutrition. In 1893, at the top of her class, she took her master's degree in physics; in 1894, her master's degree in mathematics.

About this time Marie undertook her first commission: to study the magnetic properties of steels. In the course of this work she met Pierre Curie. He was a man of thirty-five, already a highly esteemed physicist. Like Marie, he came of a most cultivated, middle-class family; like her, he was devoted to his science to the exclusion of people. The two were quickly in sympathy, and shortly they were close friends. Two years later they married.

The Curies now began an amazing collaborative work at the School of Physics and Chemistry of the City of Paris, where Pierre was chief of laboratory. Marie, searching for a subject for a doctoral dissertation, had become interested in Becquerel rays and their sources. As she systematically examined all known elements and minerals, she began to suspect that in the pitchblende (uranium ore) she had studied there was a hitherto-unidentified element capable of radiation far stronger than that from uranium. Pierre at once abandoned his work with crystals to join in the study of the Becquerel rays. In 1898 they together announced the probable existence of polonium; a few months later they announced radium.

From 1898 to 1902 they devoted themselves to the long, arduous task of preparing a sample of pure radium from the masses of pitchblende they were, with difficulty, able to obtain. Together they studied the physical and chemical properties of the new element. Finally, in 1902, Marie isolated pure radium salt and determined its atomic weight, 225.

Meantime, Pierre had become a teacher at the P.C.N., an annex of the Sorbonne, and Marie had become a lecturer in physics at the girls' normal school at Sèvres. Though these teaching duties constantly drained their energies, though

their earnings scarcely paid their modest bills, though Pierre's health failed, they never halted in their research. By 1904 they had published twenty-nine papers on radioactivity, most of them so completely joint products that the work of one is indistinguishable from that of the other.

For her work on radium Marie won her doctoral degree in 1903; in the same year the Curies began to receive the honors which, until Marie's death in 1934, never stopped coming. They visited London to present the results of their studies to the Royal Society, and Marie attended the meeting —the first woman ever admitted; they were jointly awarded the Davy Medal in 1903, and a few months later, together with Becquerel, the Nobel Prize for Physics. Even this triumph scarcely persuaded them to pause in their labors long enough to visit Stockholm for the presentation of the prize money. They scarcely paused to celebrate Pierre's election to the Academy of Science, or his elevation to a professorship at the Sorbonne. Suddenly, in 1906, after an idyllic Easter holiday in the country, Pierre died, the victim of a street accident. The great collaboration was ended.

The tremendous official and the genuine friendly sympathy which rose round Marie meant nothing to her. She was sustained only by that devotion to science which had persuaded her and Pierre several years before to refuse to patent their process for refining radium: by that devotion, and by her ingrained habit of work. Within a few months the Sorbonne entrusted her with Pierre's course, as his successor. She labored upon her lectures, and in November she delivered the first of them, beginning exactly where Pierre had left off. The first woman ever to lecture at the Sorbonne, she soon began to give the first—and for long the only—course in the world on radioactivity.

From 1906 to 1914, as the fame of Marie Curie grew, she never stopped working—hardly even for an occasional visit to Warsaw, such as that in 1913 to inaugurate a laboratory for the study of radioactivity, or for a quick trip to a foreign university to receive an honorary degree, or for a summer walking tour in the Engadine, or for a second excursion to Stockholm to receive in 1911 the Nobel Prize for Chemistry. She studied polonium exhaustively; she administered the fellowships for the study of radioactivity established by Carnegie; she prepared the first and only sample of pure metallic radium and redetermined its atomic weight; when the University of Paris and the Pasteur Institute jointly undertook the construction of an Institute for Radium, she supervised the execution of the scheme.

This building was just ready for occupancy when World War I broke out in 1914. For the next five years Mme. Curie was occupied constantly with the war work she made peculiarly hers. She observed at once that the army hospitals were not equipped to use radiology. Almost unaided, and frequently in the face of official lethargy, by the end of the war she had equipped two hundred radiological rooms, most of them in field hospitals, and twenty radiological cars. She herself performed prodigies in the field as an X-ray technician; she trained one hundred and fifty X-ray technicians, and she organized and operated the radium emanation service. Her patriotic fervor blazed: not only her services—and those of all the scientists she could commandeer—but her prize winnings and her whole fortune, she put at the disposal of the government. But immediately the war ended, she resumed her investigative studies.

Though the hard work never ended, nor her eagerness for it, Mme. Curie's life for the next fifteen years had a brighter tone than before. She had wonderful summer holidays at Larcouest, a quiet spot in Brittany, with a group of congenial academic people from the Sorbonne. She watched the progress of her daughter and her son-in-law, the Joliot-Curies, who were rising to eminence in the world of science. She even occasionally accepted the world's homage; for she came to believe that whatever was offered her was in reality a tribute to science. Thus in 1921 she made a trip to America, an almost royal progress, to receive from the women of America the gift of a gram of radium, and repeated the same tour for the same reason in 1929. (The first gift she immediately transferred to the Paris Radium Institute, and the second to the Warsaw Institute, founded in 1925.) Thus she journeyed to Rio de Janeiro, to Italy, to Holland, to England, to Czechoslovakia, to Spain. The learned societies of the world elected her to membership; the universities of the world conferred their honorary degrees upon her. She accepted everything with complete self-effacement. It even seemed to her that these expeditions, pleasurable as they sometimes were, cost overmuch in their interruptions of her work. Even when her health declined and her sight dimmed, her energy did not. Almost until the day of her death she was busy writing her last, her greatest, book. It was just finished when she died.

Mme. Curie's story has been so colorfully told by her daughter Eve—and so vividly presented on the movie screen—that everyone knows of her, and thinks of her, probably, as a fairy-tale heroine of science. Yet her genius was not romantic. It was a genius for hard work. She had a passionate devotion to accuracy, to truth, to science—a devotion which made her

all but selfless. Even when she must credit herself with her own achievements, as in the pages here translated from her last book, *Radioactivity,* she speaks of herself in the third person. For she cared nothing for personal glory, everything for labor and knowledge.

RADIOACTIVITY

THE DISCOVERY OF RADIOACTIVITY AND OF THE RADIOELEMENTS

THE STUDY of radioactivity includes the study of the chemistry of the radioelements, the study of the rays emitted by these elements, and the conclusions to be drawn from such studies relative to the structure of the atom. The radioelements can be defined as particular elements from which there emanate, spontaneously and atomically, rays designated as *alpha, beta,* and *gamma*—positive corpuscular rays, negative corpuscular rays (electrons in motion), and electromagnetic radiations. The emission is accompanied by an atomic transformation. Arranged according to their respective abilities to penetrate matter, the *alpha* rays are the weakest: they are stopped by a sheet of paper or by a leaf of aluminum 0.1 mm. in thickness; they travel through air a few centimeters. The *beta* rays travel farther in air and can penetrate several millimeters of aluminum. The *gamma* rays can penetrate several centimeters of relatively opaque material such as lead.

The Rays of Uranium. The Rays of Thorium.

Henri Becquerel discovered radioactivity in 1896.

After the discovery of Roentgen rays, Becquerel began his researches upon the photographic effects of phosphorescent and fluorescent substances.

The first tubes which produced Roentgen rays had no metallic anticathode. The source of the rays was the glass wall of the tube, rendered fluorescent by the action of the cathode rays. It was natural to inquire whether the emission of Roentgen rays did not necessarily accompany the production of fluorescence, whatever might be the cause of the latter. Henri Poincaré suggested that it did, and various attempts were made to obtain photographic impressions on plates shielded in black paper, using zinc sulphide and calcium sulphide previously exposed to light; the results were finally negative.

H. Becquerel made similar experiments with the salts of uranium, some of which are fluorescent. He obtained impressions on photographic plates wrapped in black paper with the double sulphate of uranyl and potassium. Subsequent experiment showed that the phenomenon ob-

served was not linked to that of fluorescence. The salt used need not be activated by sunlight; further, uranium and all of its compounds, whether fluorescent or not, act on the photographic plate in the same way, and metallic uranium is the most active of all. Becquerel eventually discovered that compounds of uranium, placed in complete darkness, continued for a period of years to make impressions on photographic plates wrapped in black paper. He then affirmed that uranium and its compounds emit special rays: uranium rays. These rays can penetrate thin metallic screens; as they pass through gases, they ionize them and render them conductors of electricity. The radiation from uranium is spontaneous and constant; it is independent of external conditions of light and temperature.

The electrical conductivity caused in the air or other gases by the uranium rays is the same as that caused by Roentgen rays. The ions produced in both cases have the same mobility and the same coefficient of diffusion. Measurement of the current for saturation provides a convenient means of measuring the intensity of radiation under given conditions.

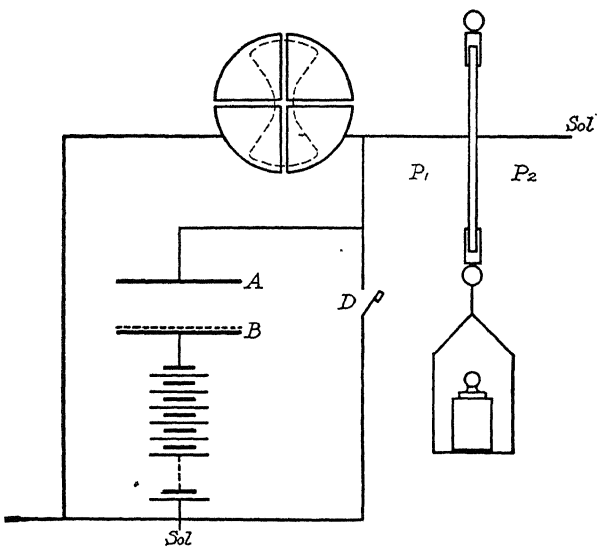
The Thorium Rays. Researches made simultaneously by G. Schmidt and Marie Curie have shown that the compounds of thorium emit rays like the uranium rays. Such rays are usually called *Becquerel rays.* The substances which emit Becquerel rays are called radioactive, and the new property of matter revealed by that emission has been named by Marie Curie radioactivity. The elements which so radiate are called radioelements.

Radioactivity an Atomic Property. New Method of Chemical Analysis Based on Radioactivity. Discovery of Radium and Polonium.

From Becquerel's researches, it was clear that the radiation from uranium is more intense than that from its compounds. Marie Curie made a systematic study of all known metallic elements and their compounds to investigate the radioactivity of various materials. She pulverized the various substances and spread them in uniform layers on plates of the same diameter which could be inserted into an ionization chamber. Using the piezo-electric quartz method, she measured the saturation current produced in the chamber between the plates A and B (see p. 578). With plates 3 cm. in diameter, placed three centimeters apart, an even layer of uranium oxide gives a current of about 2×10^{-11} amperes, which scarcely increases as the thickness of the layer increases after it exceeds a fraction of a millimeter; the emanations are almost all alpha rays of uranium, easily absorbed. Measurements made upon the compounds of uranium have certified that the intensity of radiation increases with the uranium content. The same thing is true for the thorium compounds. The radioactivity of these elements is therefore an atomic property.

On the contrary, a substance such as phosphorus cannot be considered radioactive because to produce ionization it must be in the state of

white phosphorus; in the red state, or in a compound such as sodium phosphate, it does not produce ionization. Similarly, quinine sulphate, which produces ionization only while it is being heated or cooled, is not radioactive, for the emission of ions is produced here by the variation in temperature and is not an indication of radioactivity of any one of the constituent elements. It is, indeed, a fundamental characteristic of radioactivity that it is a spontaneous phenomenon and an atomic property. These considerations played an important part in the discovery of radium.



Marie Curie carried on her measurements, using both the widely distributed elements and the rare elements, and as many of their compounds as possible. In addition to pure substances, she also examined a great many samples of various rocks and ores. For simple substances and their compounds, she demonstrated that none except thorium showed an activity equal to 1% of that of uranium.

Among the ores examined, several were radioactive: pitchblende, chalcolite, autunite, thorite, and some others. Since all of these contain either uranium or thorium, it was natural to find them active; but the intensity of the phenomenon with certain minerals was unexpected. Thus some pitchblendes (oxide of uranium) were four times as active as metallic uranium. Chalcolite (copper phosphate and crystalline uranium) was twice as active as uranium. These facts did not agree with the results from the study of simple substances and their compounds, according to which none of these minerals should have shown more activity than uranium or thorium. Furthermore, double phosphate of copper and uranium, of the same formula as chalcolite, prepared from uranium salts

and pure copper, showed an activity quite normal (less than half that of uranium). Marie Curie formed the hypothesis that pitchblende, chalcolite, and autunite each contain a very small quantity of a very strongly active material, different from uranium, from thorium, and from already known elements. She undertook to extract that substance from the ore by the ordinary processes of chemical analysis. The analysis of these ores, previously made in general to an accuracy of nearly 1% or 2%, did not destroy the possibility that there might occur in them, in a proportion of that order, a hitherto unknown element. Experiment verified the prophecy relative to the existence of new, powerfully radioactive radioelements; but their quantity turned out to be much smaller than had been supposed. Several years were required to extract one of them in a state of purity.

The research upon the radioelement hypothesized was made first by Pierre Curie and Marie Curie together, using pitchblende.

The research method had to be based upon radioactivity, for no other property of the hypothesized substance was known.

Radioactivity is used in a research of this kind in the following way: the activity of a product is measured; it is then subjected to chemical separation; the radioactivity of each resulting product is measured, and it is observed whether the radioactive substance now remains integrally in one of the products or is divided among them, and if so, in what proportion. The first chemical operations carried out showed that an enrichment in active material was possible. The activity of the solid products—well-dried and spread in a powdered state on plates—was measured under comparable conditions. As more and more active products are obtained, it is necessary to modify the technique of measurements. Some methods of quantitative analysis for radioactive materials will be described later on in this work.

The method of analysis just described is comparable to spectrum analysis from low to high frequencies. It not only discovers a radioactive material, but also distinguishes between the various radioactive elements. For they differ from one another in the quality of their radiations and in their length of life.

The pitchblende from St. Joachimstahl which was used in the first experiments is an ore of uranium oxide. Its greatest bulk is uranium oxide, but it contains also a considerable quantity of flint, of lime, of magnesia, of iron, and of lead, and smaller quantities of some other elements: copper, bismuth, antimony, the rare earth elements, barium, silver, and so on. Analysis made by using the new method showed a concentration of the radioactive property in the bismuth and in the barium extracted from the pitchblende. Yet the bismuth and the barium in commercial use, which are extracted from non-radioactive ores, are not themselves active. In agreement with the original hypothesis, Pierre and Marie Curie concluded that there were in the pitchblende two new radioactive elements: polonium and radium. The first of these they took to be analogous in its chemical properties to bismuth, and the second to barium.

They announced these conclusions in 1898. At the same time, they indicated that polonium could be separated from the bismuth by such chemical treatments as the fractional precipitation of the sulphides or the nitrites, and that radium could be separated from barium by the fractional crystallization of the chlorides in water, or their fractional precipitation by alcohol. Theoretically, they claimed, such processes should lead to the isolation of the new radioelements.

A specimen of radium-bearing barium chloride, sixty times as active as the oxide of uranium, was submitted to spectral analysis by Demarçay. He found, accompanying the spectrum of barium, a new line of 3815 Ångstrom units. Later, examining a specimen nine hundred times as active as the oxide of uranium, he found the line of 3815 Å. much strengthened, and two other new lines. Examination of polonium-bearing bismuth, though the specimen was very active, revealed no new lines.

It had become clear that the new elements occurred in the ore in very small proportions, and that they could be isolated only by treating hundreds or even thousands of kilograms of the ore. To accomplish this labor, it was necessary to have recourse to industrial operations, and to treat the concentrated products thus obtained. After several years, Marie Curie succeeded in obtaining several decigrams of a pure radium salt, in determining the atomic weight of that element, and in assigning to it a place in the periodic table hitherto vacant. Still later, Marie Curie and A. Debierne isolated radium in the metal state. Thus the chemical individuality of radium was established in the most complete and rigorous way.

The application of the new method of investigation later led to the discovery of other new radioelements: first, actinium (discovered by A. Debierne), then ionium (by Boltwood), then mesothorium and radiothorium (by O. Hahn), then protoactinium (by O. Hahn and L. Meitner), etc. There have also been identified radioactive gases called emanations.

Among all these substances, radium is the most widely known and most widely used. Practically unvarying because of the slowness of its transformation, it is now industrially prepared, especially because of the medical applications of the gamma radiations to which it apparently gives rise, and which are, in reality, only indirectly attributable to it. Radium produces, apparently continuously, a radioactive gas named radon, and this gas gives birth to a series of substances: radium A, radium B, radium C. The last of these emits particularly penetrating gamma rays. Radium and the derivatives which usually accompany it furnish intense sources of alpha, beta, and gamma radiations. These have been and are the principal ones used in researches upon such radiations. From the point of view of chemistry, the studies of radium have confirmed the atomic theory of radioactivity and have provided a solid foundation for a theory of radioactive transformation.

Spectrum and Atomic Weight of Radium. Metallic Radium.

Since radium is an alkaline-earth metal, it is extracted from its ores simultaneously with the barium also found there, or combined with it. The mixture of radium and barium is submitted to a series of operations of which the result is the separation of the radium from the barium in the form of a pure salt.

As the products of these operations are successively enriched in radium, their radioactivity increases, the intensity of the spectral lines for radium increases—as compared with the barium lines—and the mean atomic weight increases. When the radium salt is wholly pure, the photographed spark spectrum shows only the lines characteristic of radium; the strong 4554.4 Å line of barium, of such sensitivity that it is extremely hard to eliminate, is now scarcely discernible.

A radium salt introduced into a flame gives it a carmine-red color, and produces a visible spectrum composed of the characteristic radium lines (Giesel).

In general, the appearance of the radium spectrum resembles that of the alkaline-earth metals. It includes bright, narrow lines and also cloudy bands. The principal lines of the spark spectrum and of the flame spectrum follow:

Spark Spectrum		*Flame Spectrum*	
4821.1	faint	6653	
4682.3	very bright	6700–6530	band
4533.3	faint	6329	
4340.8	bright	6330–6130	band
3814.6	very bright	4826	
3649.7	bright		
2814.0	bright		
2708.6	bright		

The spark spectrum shows two bright, nebulous bands, with maximum intensity at 4627.5 and 4455.2 Å respectively.

The spectral reaction of radium is very sensitive. It makes possible the identification of radium present in a substance in the proportion of 10^{-5}. But the radioactive reaction is still more sensitive; it makes possible the identification of the radium when its concentration is no more than 10^{-12}.

The atomic weight of radium, or the mean atomic weight of a mixture of radium and barium, can be determined, as for barium, with precision. Although the radioactivity of the mixture is not less than 1000 times that of uranium, its atomic weight differs only negligibly from that of barium.

The method used to make this determination is as follows: chloride of radium, the purity of which had been certified by spectral analysis, was

deprived of its water of crystallization at a temperature of about 150° C. and was carefully weighed in the state of an anhydrous salt. From a clear solution of this salt, the chlorine was precipitated as silver chloride, and the silver chloride was weighed. From the relation of that second weight to the first, supposing that the formula for anhydrous chloride of radium is $RaCl_2$—by analogy with the formula $BaCl_2$, accepted for barium chloride—and using the accepted atomic weights of chlorine and silver, the atomic weight of radium could be calculated.

The details of this technique have been explained in special reports (Marie Curie, E. Hoenigschmid). The quantities of the chloride of radium used have varied from 0.1 gm. to 1.0 gm., and the various determinations have resulted uniformly. Taking the atomic weight of silver as 107.88 and that of chlorine as 35.457, the atomic weight of radium is 226 (Hoenigschmid).

To isolate radium in its metallic state, the amalgam of radium was prepared by electrolyzing, with a cathode of mercury, a solution containing 0.1 gm. of pure radium chloride. The resulting liquid amalgam decomposes water and is modified by air. It was dried, placed in a vessel of pure iron, and distilled in an atmosphere of pure hydrogen obtained by osmosis through incandescent platinum. The amalgam solidified at about 400° C. The metal, cleared of mercury, melts at 700° C. and begins to volatilize. Radium is a white, shining metal which rapidly alters in air, and which decomposes water energetically.

In accord with its atomic weight, radium has been placed in the periodic table of the elements as a higher homologue of barium, in the last line of the table; its atomic number is 88; its spectrum and its chemical properties accord with its position; similarly with its high-frequency spectra (values of L_1 and L_2 levels) (Maurice de Broglie).

Here is a résumé of the chemical properties of the radium salts: the sulphate is insoluble in water and the common acids (solubility in water at 20° C. is 1.4×10^{-3} gm. per liter); the carbonate is insoluble in water and in solutions of alkaline carbonates; the chloride is soluble in water (at 20° C., 245 gm. of $RaCl_2$ per liter), insoluble in concentrated hydrochloric acid and in pure alcohol; the bromide behaves similarly (at 20° C., 706 gm. of $RaBr_2$ per liter); the hydrate and the sulphide are soluble. The separation of radium from barium by fractional crystallization depends upon the fact that the chloride and the bromide of radium are less soluble than the corresponding salts of barium (at 20° C., 357 gm. of $BaCl_2$ and 1041 gm. of $BaBr_2$ per liter of water).

The Radioelements

Each radioelement undergoes a transformation consisting of the successive destruction of all its atoms, in accord with a law that half the number existing at a given moment are transformed in a time T which is characteristic of the radioelement under consideration, and which is called

its *period*. Measured by the magnitude of the period, radioelements have a life which is more or less long. Some, like uranium and thorium, which have survived several geological epochs in the ores which contain them, have a very long life. Others, such as radium, actinium, polonium, mesothorium, radiothorium, and so on, would have disappeared wholly from the ores if their decay had not been compensated for by their production from uranium and thorium. These two primary elements form, therefore, the heads of series or families to which belong all the other radioelements —derivatives of the two, bound to one another by lines of descent. The quantities of the derived elements which exist in untreated ores are proportional to the quantities of the primary elements there, and to the periods of the derivatives. Each derived element with a life sufficiently long can be extracted from uranium and thorium ores, just as the primary elements are; but sometimes it can be obtained by the decay of a more or less distantly related element which has already been extracted from the ore. For the radioelements of short life, only the latter method is available. In this chapter are given descriptions of the radioelements in the order which they occupy in the several families.

The chemical properties of uranium and of thorium have been described in treatises on chemistry, and will be omitted here. There exist at least two isotopes of uranium, U_I (period of the order of 10^9 years) and U_{II}, a derivative with a very short life, existing in small proportion along with U_I. There is probably also a third isotope, AcU.

The Derivatives of Uranium

A. THE RADIUM BRANCH

Uranium X. The compounds of uranium emit alpha, beta, and gamma rays; always, the alpha rays come from the uranium itself (U_I and U_{II}); the penetrating beta and gamma rays are emitted by a group of derivatives which together form Uranium X, discovered by Crookes. Experiments show that the alpha-radiating material cannot be separated from the uranium; but by various reactions, the material which emits the beta and gamma radiations can be separated from the uranium. The methods of operation most employed are the following: fractional crystallization of uranium nitrate, extraction of the uranium from solution by the addition of ammonium carbonate in excess, and the treatment with ether of a highly concentrated solution of uranium nitrate. In the first process, uranium X is concentrated in the more soluble portions. In the second, uranium passes into solution, and the uranium X remains, with insoluble impurities such as iron, in the alkaline solution. In the third, two layers of the liquid form; the one richer in ether contains a solution of uranium without uranium X; the one richer in water contains uranium X in excess. The active material thus separated has a period of twenty-four days.

Uranium X is not simple, but is composed of several radioelements.

The substance with a period of twenty-four days, preparation of which has just been described, is an isotope of thorium (atomic number, 90), and is called uranium X_1; it is produced by U_I, and it emits a group of beta rays only mildly penetrative.

Uranium X_1 gives rise to a derivative of very short life, uranium X_2 or brevium (Fajans and Goehring). Its period is 1.13 minutes; it is a higher homologue of tantalum (atomic number, 91); it emits a group of penetrating beta rays. Finally, there are found in uranium X, in very small proportions, two other radioelements: uranium Y (Antonoff), an isotope of thorium (atomic number, 90; period, 25 hours); and uranium Z (Hahn) (atomic number, 91; period, 6.7 hours).

Ionium. Ionium, discovered by Boltwood, is the derivative of uranium which is transformed directly into radium. Its period is 83,000 years. Its chemical properties are exactly those of thorium, the two elements being isotopes (atomic number, 90). In the treatment of ores, ionium is found in the same portions as the thorium, and it is separated at the same time as that rare earth element. From the uranium ore, what is actually extracted is, therefore, a mixture of thorium-ionium; and though the proportion of ionium is generally smaller than that of thorium, it may be comparable to it.

The spectrum of a thorium-ionium mixture containing 30% of ionium is identical with that of thorium. This fact has been taken as an argument that the spectra of isotopes are identical. Later researches into the isotopes of lead have shown, however, that the identity is not complete; there are very minute differences.

Though ionium occurs in relatively important quantities in the uranium ores (perhaps 20 gm. per ton of uranium), it cannot be extracted as a pure salt because of its close association with thorium.

The radiation of ionium is simple; it is composed principally of alpha rays accompanied by a weak gamma ray of little penetrative power.

Radium and its first derivatives. The chemical individuality of radium has already been given in earlier sections. Its period is 1600 years. By radioactive transformations, radium produces a series of short-lived radioelements by which it is generally accompanied. These are a radioactive gas, or emanation from radium, called radon, and the components A,B,C,C′,C″ of the active deposit. The radiation of this group is complex, and is composed of alpha, beta, and gamma rays.

Radium D. Radium E. Radium D is an isotope of lead (atomic number, 82; period, 22 years). It emits a beta radiation of which the ionizing power is very small; its presence is revealed by the formation of derivatives. Of these derivatives, the first, radium E (isotope of bismuth; atomic number, 83; period, 5 days), has a beta radiation; the second, radium F, identical with polonium, has an alpha radiation. Radium D can be extracted from uranium ores at the same time as the lead which they con-

tain, and cannot be separated from this lead. This radioactive lead—or radiolead—can be used as the primary material for the preparation of polonium. Radium D can also be obtained from radium, from which it derives through the intermediary steps of radon and the materials of its active deposit.

Polonium. Polonium is the first radioactive element discovered by the new method of chemical analysis based on radioactivity. It is a derivative of uranium through the intermediary stage of radium. It is characterized by an alpha radiation, and by the absence of penetrating rays. Its presence was recognized in the sulphides precipitated in an acid solution of pitchblende, and, in the analysis of these sulphides, it particularly clung to the bismuth. By means of the fractional precipitation of the bismuth salts from water, the polonium can be concentrated in the less soluble portions. Later research has shown that this element occurs in the ores in a much smaller proportion than radium, and that it decays, with a period of 140 days. Marckwald has demonstrated that in certain of its chemical properties, polonium is analogous to tellurium. It is characterized also by the ease with which certain metals (iron, copper, silver) displace it from acid solutions. It can be prepared either from ores or from radiolead or from radium.

The largest quantity of polonium hitherto prepared (Marie Curie and A. Debierne) consists of about 0.1 mg. mixed with several milligrams of foreign metals easily reducible. The radiation of that sample was comparable to that of 0.5 gm. of radium. Among the lines in the spark spectrum, there was one (4170.5 Å) which seems to belong to polonium. More recently, there has been announced the existence of a line of 2450 Å (A. Czapek).

To polonium, in the periodic table, has been assigned a place, hitherto vacant, beside bismuth (atomic number, 84), as a higher analogue of tellurium.

The analogy which polonium presents in part with tellurium, in part with bismuth, is explainable, apparently, on considerations of valency. For the compounds in which polonium is trivalent (sulphide), the analogy with bismuth is valid; for those in which it is tetravalent (chloride, hydroxide), the analogy with tellurium is valid (M. Guillot). Polonium is soluble in acid solutions and also in concentrated soda solutions. It can behave, then, like a metal, or it can enter, like tellurium, into an acid radical. In solutions almost neutral, its compounds undergo hydrolysis and the radioactive material is deposited on the walls of the container; this process is hastened by centrifugation. Polonium appears to be susceptible to linkage in certain complexes such as chloropoloniate of ammonia, an isomorph of the corresponding salts of iron, lead, strontium, platinum; or the diethylthiosulfocarbonate of polonium, an isomorph of the salt of cobalt having the same formula. Experiment in electrolysis points to ions of complex form.

Polonium can be volatilized, and the distilled material can be caught

by a gas current. The purest preparation so far obtained upon a small surface corresponds, according to numerical evaluations, to more than fifty molecular layers, superimposed; the color is gray or black, attributable to polonium or to one of its oxides. Some polonium compounds, such as the hydride and the polonium carbonyl have been reported to be particularly volatile.

B. THE ACTINIUM BRANCH

The elements of the actinium family are, in all probability, derivatives of uranium; but they are not of the same linked series as radium and its derivatives. It is supposed that the isotopes of uranium give rise to two lines of derivatives, of which the radium family forms one and the actinium family the other. The first certainly known member of the latter family is protoactinium. The connection between protoactinium and uranium is probably through the intermediary UY.

Protoactinium (Hahn and Meitner, Soddy and Cranston). Protoactinium was discovered in the residue remaining from the treatment of pitchblende from St. Joachimstahl. It is the immediate parent of actinium. It emits alpha and beta rays, and it has a period of 30,000 years. In certain of its chemical properties it is analogous to tantalum, of which it is the higher homologue (atomic number, 91). But according to the experiments of Grosse, its oxide, instead of having the properties of an acid, behaves rather like a weak base. Grosse has perfected a method of fractional crystallization of the chlorides of zirconium and of protoactinium, the latter concentrating in the solution, and has obtained several centigrams of the radioelement in a pure state. Protoactinium occurs in the ores of uranium in a proportion comparable to that of radium, and can be extracted in sufficient quantity to determine its atomic weight.

Like tantalum, protoactinium can easily be dissolved as an oxide or hydrate in hydrofluoric acid. The oxide (probable formula, Pa_2O_5) is a white powder with a high fusion point; calcined, it is insoluble in hydrochloric, nitric, sulphuric acids. By fusion with $NaSO_4$ and recovery by water and sulphuric acid, it can be dissolved and separated from tantalum. After fusion with K_3CO_3 and recovery by water, protoactinium remains in the insoluble residue, whereas the tantalum dissolves. In a hydrochloric, nitric, or sulphuric solution, the protoactinium can be precipitated entirely by an excess of phosphoric acid.

Actinium. Actinium (A. Debierne) belongs, according to its chemical properties, among the rare earth elements. Extracted from ore at the same time as the elements of this group, it can be separated only by laborious fractionations. Its presence is revealed by the radiation of its successive derivatives. These are formed so slowly that the activity of actinium freshly prepared increases for several months. The period of actinium

being about ten years, it forms with its derivatives a relatively stable group (actinium family) with a complex alpha, beta, gamma radiation.

Like polonium and radium, actinium was first found in pitchblende. This generally contains, in a small proportion, rare earths, principally of the cerium group: cerium, lanthanum, neodymium, praseodymium, samarium; there are also always small quantities of thorium. In this mixture of substances with neighboring properties, thorium is the element most weakly basic, and lanthanum the one most strongly basic. Actinium is especially close to lanthanum and is even more strongly basic.

Actinium is precipitated with thorium and with the rare earth elements in the state of hydrates, fluorides, or oxalates (the precipitation being relatively less complete than for lanthanum). It remains with the other rare earths when thorium and cerium are separated from them by the usual methods. The rare earths can be separated from one another by the methodical fractionation of their double ammoniacal nitrates in a nitric solution. The actinium comes out at the same time as the lanthanum, in the least soluble fractions. To enrich the actinium-bearing lanthanum in actinium, there has been used successfully the fractional precipitation of the oxalate in a nitric solution; the actinium concentrates in the solution (Marie Curie and collaborators). By applying this method to the actinium-bearing lanthanum extracted from uranium ore from Haut Katanga, several grams of the oxide, containing 1 to 2 milligrams of actinium, have recently been obtained; this quantity corresponds in the ore to about ten tons of uranium.

The isomorphism of the salts of actinium and lanthanum being demonstrated by the regularity of the fractional crystallizations, it can be supposed that the chemical formulas of the actinium compounds are of the same type as the corresponding formulas for lanthanum.

In the periodic table, there has been assigned to actinium a place, hitherto vacant, in the column of the trivalent elements, in the last line of the table (atomic number, 89).

Radioactinium. Actinium X. These substances are the first derivatives of actinium and are obtained by beginning with it. Radioactinium (Hahn) is an isotope of thorium (atomic number, 90), with a period of 18.9 days; it emits an alpha radiation and also weak beta and gamma radiations. It can be separated from actinium by the same methods used to separate thorium from lanthanum. It gives rise to the formation of actinium X (Giesel, Godlewski), which has a period of 11.2 days and a radiation like that of radioactinium. Actinium X is an isotope of radium (atomic number, 88). From a solution containing actinium, radioactinium, and actinium X, the first two can be separated by precipitating them with ammonia; the actinium X remains in solution. Actinium X gives birth to *actinon* (a radioactive gas), which produces an *active deposit* from actinium composed of a number of constituents.

The Derivatives of Thorium

Mesothorium 1. This substance, discovered by O. Hahn, accompanies the radium extracted from ores which contain uranium and thorium (thorianite, monazite). The beta and gamma radiations which it appears to give really come from a short-lived derivative of it, mesothorium 2. The latter can be separated from the former by precipitation by ammonia, and it immediately re-forms. Mesothorium 1 gives off no measurable radiation. It has not been separated from radium, of which it is an isotope (atomic number, 88); its period is 6.7 years. Its use in medicine is analogous to that of radium, and it has been industrially extracted as a by-product of the preparation of thorium in the incandescent-mantle industry.

Mesothorium 2 is an isotope of actinium (atomic number, 89), and though its period is only 6.2 hours, it has nevertheless been possible to study its chemical properties (Yovanovitch). Thence has been learned much about the chemical properties of actinium, the study of which, as has been observed, involves great delays. This is an example of the method of radioactive indicators.

To separate mesothorium 2 from mesothorium 1, the method is currently used of crystallization in a strongly acid hydrochloric solution in the presence of barium. This operation leaves mesothorium 2 in solution while the chloride of mesothorium 1 crystallizes with the barium-chloride.

Mesothorium is a source of radiothorium. After the solution has been for some time undisturbed, that substance accumulates, and can be separated by NH_3 after the addition of several milligrams of another reagent. In the crystallization hitherto described, radiothorium accumulates in the solution with mesothorium 1; but if the operation is repeated several times at intervals of a day, finally mesothorium 2 quite free of radiothorium collects, the speed of formation of these two being different.

Radiothorium. Thorium X. Radiothorium was found by O. Hahn in thorianite from Ceylon of which some hundreds of kilos had been submitted to treatment for the extraction of radium. This ore is composed chiefly of thorium oxide, but contains also some uranium oxide, and, consequently, some radium. When the chloride of radium-bearing barium coming from this mineral was submitted to fractional crystallization, it was remarked that at the same time that the radium concentrated in the less soluble portions, another radioactive substance concentrated in the more soluble portions. This material had the radioactive properties of thorium, but in a heightened degree; in particular, it gave off in great quantities the radioactive gas which is obtained from thorium compounds and which is called *thoron,* or *thorium emanation.* The new radioelement responsible for this release of gas has been called radiothorium. It is now known that it is present in the compounds of thorium as a derivative. Radiothorium has also been discovered in the deposits of some hot springs in Savoy

(Blanc). Radiothorium is an isotope of thorium (atomic number, 90); its period is 1.9 years. Its radiation is made up chiefly of alpha rays, but it also feebly gives off beta rays. It produces a short-lived derivative, thorium X (Rutherford, Soddy) (isotope of radium, period of 3.64 days, alpha and weak beta radiation), which is used in medicine. It can be separated from a solution of radiothorium by precipitating the latter with ammonia or with oxygenated water; thorium X remains in solution. Thorium X is the direct parent of thoron, from which come other derivatives forming its active deposit.

THE RADIOACTIVE ORES AND THE EXTRACTION OF THE RADIOELEMENTS

The Radioactive Ores

THESE ORES, of which a large number are known, are all ores of uranium and thorium, containing these two elements in varying proportions, in association with inactive elements. Sought for more actively since the discovery of radium, they have been found in different parts of the globe. The radioelements, derivatives of uranium or of thorium, occur in the ores in quantities proportional to those of the primary substances, respectively. Among the exploitable ores of uranium, some are almost free of thorium and contain only the series of derivatives which begin with uranium; the radium which is extracted from them is free of mesothorium. On the contrary, the commercial ores of thorium contain an appreciable quantity of uranium; with the descendants of thorium there are also present those of uranium. The mesothorium obtained in industry is therefore always accompanied by radium. For equivalent radiation, such a mixture is less valuable than radium, for mesothorium decays in accord with its period of 6.7 years, whereas radium is practically constant, its period being 1600 years.

The radioactive ores occur sometimes in a concentrated form, but more frequently in a dispersed form. In the first case, they form crystals of considerable volume, or compact masses which are found as threads or beads embedded in massive rock. In the second case, they are intimately mixed through rock or soil which they impregnate wholly, or through which they are disseminated in the form of extremely tiny crystals. Industrially, not only the rich ores—containing 50 milligrams or more of radium per ton—but also the poorer ores—containing only a few milligrams of radium per ton—have been successfully used. In the ores, the relation between the quantity of radium and that of uranium has a constant value of 3.4×10^{-7}. Consequently, no ore can possibly contain more than 340 milligrams of radium per ton of uranium.

To recognize that an ore is radioactive, two simple processes are available: 1. A piece of the ore is placed on a photographic plate which is kept entirely in darkness for a day before it is developed. In the image

obtained, the dark portions correspond to the active portions of the specimen, and the light portions to the inactive parts. 2. A piece of the ore may be pulverized, the powder so obtained placed upon a plate, and the ionization produced by the specimen measured in an electrical apparatus. Both processes are used in prospecting, and for that purpose there is available a portable electroscope. The primary, compact ores of uranium, composed of uranium oxide more or less pure, are black and dense; those in which the uranium is accompanied by acids—tantalic, niobic, titanic (samarskite, betafite, etc.)—are similarly black or dark brown. But there are also uranium ores of more recent origin, the result of the alteration of primary ores (autunite, chalcolite, curite, etc.) which are vividly colored. The thorium ores are generally of a more or less dark brown (thorite, orangite, thorianite, monazite, etc.).

Below is given a table showing a certain number of the ores, and later are recited the principal points in the treatment of first the uranium ores and then the uranium and thorium ores.

A. Ores of the oxides of uranium or of uranium and thorium:

Pitchblende (uraninite), possibly containing 30% to 80% of uranium in the form of the oxides UO_2 and UO_3, with little or almost no thorium, but with a great number of other materials in small quantities: SiO_2, Fe, Ca, Ba, Sb, Cu, Pb, Bi, etc. Compact or cryptocrystalline structure (St. Joachimstahl, England, United States, Belgian Congo, Canada).

Broggerite, cleveite, etc. Ores of crystallized uranium oxide, possibly containing thorium oxide, ThO_2, in varying proportions (Norway, United States).

Thorianite, an ore of the crystalline oxide of uranium and thorium with a great predominance of thorium (e.g., Th, 65%, U, 10%) (Ceylon).

B. Ores of hydrated deterioration:

Becquerelite (UO_3 $2H_2O$), 72% U (Belgian Congo).

Curite ($2PbO$ $5UO_3$ $4H_2O$), lead uranate, 55% uranium (Belgian Congo).

Kasolite ($3PbO$ $3UO_3$ $3SiO_2$ $4H_2O$), 40% uranium, silicouranate of lead (Belgian Congo).

C. Hydrated silicates:

Soddite ($12UO_2$ $5SiO_2$ $14H_2O$), 72% uranium (Belgian Congo).

Orangite, 66% thorium, 1% uranium (Norway).

Thorite, 45%–65% thorium, 9% uranium (Norway).

D. Phosphates:

Autunite (Ca $2UO_4$ $2PO_4$ $8H_2O$), phosphate of calcium and uranyl, about 50% uranium, in green crystalline spangles (Portugal, Tonkin).

Chalcolite, torbernite (Cu $2UO_2$ $2PO_4$ $8H_2O$), phosphate of copper and uranyl, about 50% uranium, in green crystals (Cornwall, England; Portugal).

Monazite, phosphate of rare earths, principally ceric ($CePO_4$), containing thorium (of the order of 10%) and a little uranium (of the order of 1%) (Brazil, United States, India).

E. Vanadates:

Carnotite, vanadate of UO_3 and hydrated K, about 50% uranium, in yellow crystalline powder (United States).

Ferghanite, Tuyamunite, composed of UO_3 and V_2O_5, about 50% uranium (Turkestan).

F. Niobates, tantalates, titanates:

Samarskite, niobate and tantalate of rare earths (especially the yttrium group), 3% to 15% uranium, 4% thorium (Russia, United States, India, Madagascar).

Euxenite, niobate and titanate of rare earths (yttrium), 3% to 15% uranium, 6% thorium (Norway, United States, Madagascar).

Betafite, titanoniobate and tantalate of uranium, crystallized, 25% uranium, 1% thorium (Madagascar).

Uranium Ores Containing a little Thorium. Treatment of Pitchblende

The principal ores which the radium industry has used are pitchblende, autunite, carnotite, betafite. Some of these contain so little thorium that the Th/U ratio is of the order of 10^{-5} (pitchblendes from St. Joachimstahl and from Haut Katanga). In betafite, the ratio is higher, 1 to 4%. The St. Joachimstahl pitchblende is the ore in which were discovered polonium and radium; exploited first for uranium, it was later exploited for radium. It occurs in association with dolomite and quartz in veins located at great depths (500 meters and more) in the granite mass of the region. Its composition is complex and variable; here is an example:

U_3O_8	76.82
F_2O_3	4.0
PbO	4.63
Bi_2O_3	.67
As_2O_5	.82
ZnO	.22
MnO	.04
SiO_2	5.07
CaO	2.45
MgO	.19
K_2O	.28
Na_2O	1.19
Rare earths	.52
H_2O	3.25
S	1.15
Thorium	traces

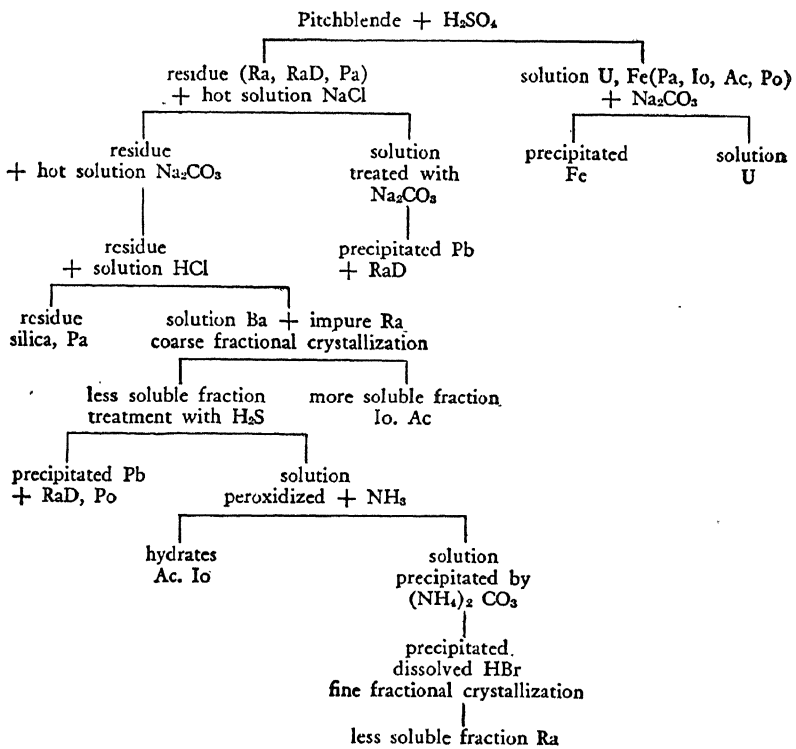
The pitchblende from the Belgian Congo (Haut Katanga) occurs in nuggets within sedimentary rocks; it is accompanied by ores resulting from the alteration of pitchblende under the action of various physical and chemical agents: chalcolite, kasolite, etc. These ores are treated in the Oolen plant in Belgium and actually provide the chief source of radium. In Canada, pitchblende has been found in lengthy veins, in ancient sedimentary rock near the Arctic Circle.

The principal phases in the extraction of radium are the following: 1. Reduction of the ore to which has been added previously a proper amount of barium to serve as a radium capturer. 2. Separation of the

crude sulphates containing the radium-bearing barium. 3. Purification of the crude sulphates and transformation of the radium-bearing barium into a chloride. 4. Fractional crystallization of the chloride to obtain a salt enriched in radium. 5. Purification of the enriched chloride and final fractional crystallization of the chlorides or bromides.

These operations are represented in the accompanying table with an indication of the products of the treatment in which certain radioelements are concentrated. It must be observed that this treatment is adapted to its principal objective—the extraction of radium and of uranium. The other radioelements, of which less accurate account is given, are dispersed in the course of the operations. (See Table I.)

TABLE I

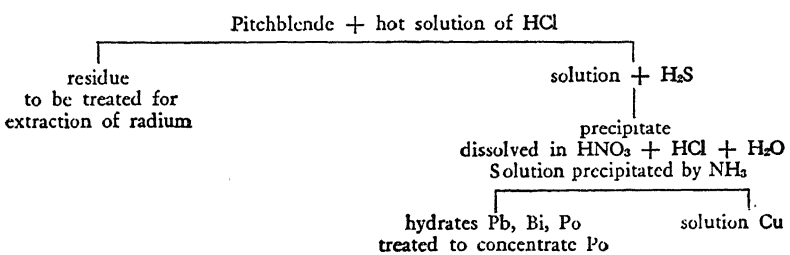


Pitchblende is generally reduced by the use of weak sulphuric acid; but that operation must sometimes be preceded by a preliminary treatment such as the roasting of the ore finely ground and mixed with carbonate of soda.

The fractional crystallization of the chlorides (the method originated by Marie Curie) is a fundamental step in the treatment. It is accomplished at first in an aqueous solution. As the extraction of the radium salt advances, it is desirable to crystallize it in a solution of increasing acidity, partly to decrease its solubility, partly to aid in the elimination of various impurities (iron, calcium, rare earth elements). Generally, the fractional crystallization is not continued until a pure radium salt is obtained, but is stopped when a concentration fixed by the use to which the product is to be put (50% to 90%) has been reached. To enrich the concentrated products, fractional crystallization of bromides replaces that of chlorides (Giesel).

The method of treating pitchblende in order to obtain polonium, used in some attempts in that direction, is given in an accompanying table. The separation of polonium with lead, bismuth, and other easily reducible metals is accomplished by making use of the chemical and electrochemical properties of polonium described earlier. (See Table II.)

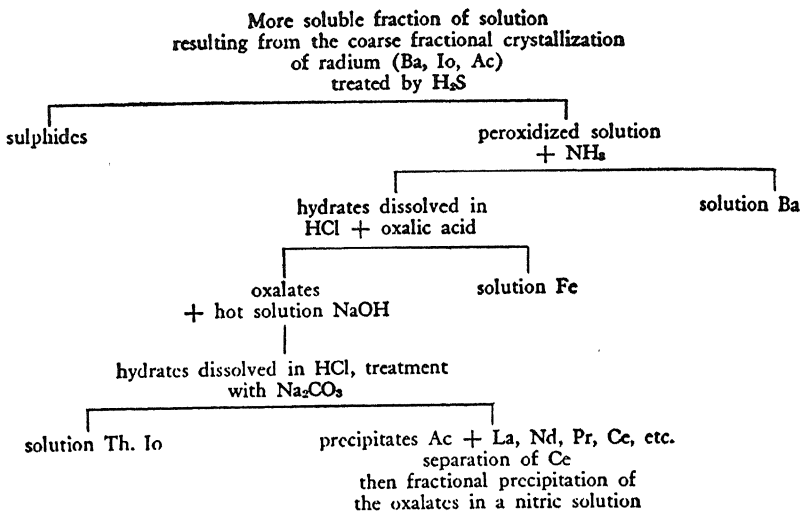
TABLE II



From Table I, it is clear that radiolead (lead + RaD) is a by-product in the preparation of radium; its separation from the ore is generally sufficiently complete, and the concentration in Radium D is greater as the ore contains less inactive lead. This radiolead may be conserved for the preparation of polonium. The method of concentration involves the following steps: 1. The precipitation of lead in a nitric solution by concentrated hydrochloric acid, leaving the polonium in solution; 2. The deposit of polonium by electrochemical means upon copper or silver leaves plunged into the solution of radiolead; 3. The capture of polonium with a precipitate of colloidal ferric hydrate.

Among the other by-products in the preparation of radium, protoactinium occurs either with the final residue of the reduction—composed principally of silica—or in the sulphuric solution of uranium. Ionium and actinium also occur—in part in that same solution, in part in the insoluble sulphates. The accompanying table records the method used to extract that material, on one hand the mixture thorium-ionium, on the other, actinium associated with lanthanum. In this treatment, hydrofluoric acid may be substituted for the oxalic acid. (See Table III.)

TABLE III



Only a few indications of the treatment used for other ores which have been exploited industrially are given here. The principal phases of the treatment are the same as for pitchblende, but the processes employed for the reduction of the ore and the obtaining of the crude sulphates may vary from one ore to another.

Carnotite—a vanadate of uranium found principally in the United States—and autunite—a phosphate of uranium and lime which has been mined principally in Portugal—can both, in certain cases, be treated with weak, hot hydrochloric acid; from that solution, the crude sulphates are precipitated. In other cases there is an advantage in treating the ore with carbonate of soda prior to dissolving it in acid.

Betafite—an ore from Madagascar which contains uranium with niobic, titanic, and tantalic acids—is reduced by fusion with soda and carbonate of soda in order to cause the rare acids to pass into solution. The reduction can also be accomplished with bisulphate of soda and reclamation with water; the sulphate of radium-bearing barium then remains in the residue with the rare acids. These latter can be separated by treatment with soda or with weak hydrofluoric acid.

Ores of Thorium and Uranium

Some ores of thorium are poor in uranium, and consequently have a scientific interest from the fact that they contain almost solely the derivatives of thorium; this is the situation with certain thorites. But in the

ores which have been exploited (thorianite, monazite) the proportion of uranium to thorium is sufficiently large for the derivatives of these two elements to be represented by comparable radiations.

Thorianite is an ore rich in thorium, found in the island of Ceylon in the form of small crystal cubes. By the treatment of several hundred kilograms of that ore, mesothorium and radiothorium were discovered. The proportion of thorium in this ore runs as high as 60 to 80%; that of uranium, 10 to 20%. Monazite, though it is less rich in thorium, is nevertheless regularly exploited for the incandescent-mantle industry, because it is found in great quantities in the so-called monazite sands of the United States and of Brazil.

Monazite is a rare-earth phosphate, crystallized, containing generally 6 to 12% of thorium. It is reduced with hot sulphuric acid, and all the soluble sulphates are extracted; in the insoluble sulphates, along with barium, radium and mesothorium 1 occur. The latter treatment of these crude sulphates does not differ in principle from that already described. The fractional crystallization is undertaken to separate in the less soluble portions the radium and the mesothorium 1 and, in the more soluble portions, the radiothorium—a disintegration product of mesothorium. The fractional crystallization can be continued until there is obtained a chloride or a bromide of radium quite free of barium and containing a negligible amount of mesothorium. After that, continued fractional crystallization does not alter the product thus obtained. The effect of the mesothorium is, however, so important that in certain products a month old it is estimated that about 75% of the most penetrating gamma rays are due to the mesothorium (through its derivative MThII) and about 25% of the most penetrating gamma rays to the radium (through its derivative RaC). The gamma radiation increases constantly for about three years because of the formation of radiothorium and its later derivatives. Having passed a maximum, it lessens because of the destruction of the mesothorium 1; after about fifty years, the radiation is due almost solely to radium, with a diminution of about 2% of the original quantity of radiation.

RELATIVITY: THE SPECIAL AND GENERAL THEORY

by

ALBERT EINSTEIN

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ALBERT EINSTEIN

1879–

EINSTEIN's is undoubtedly the best known of contemporary scientists' names. His love of music and his skill as a violinist forgotten, his devotion to philanthropic causes and his services to international order neglected, he has come to be regarded as the type of the scientist who lives in an intellectual atmosphere so rarefied that the layman dare not enter it. He is widely believed to have made science so abstruse and complicated that it retreats ever farther from the ambitious grasp. Yet he himself considers his object to be the increase of scientific clarity and simplicity.

Einstein was born in 1879 in Ulm, Württemberg, where his father owned a small electric-technical plant. In Munich he attended the Luitpold Gymnasium until 1894; then his family moved to Milan, and he entered the Cantonal School at Aarau, Switzerland. Two years later he began to attend lectures at the Technical Academy in Zurich, and shortly afterward, while still a student, he taught mathematics and physics in the same school. In 1901 he became a Swiss citizen, thus qualifying for a post as examiner of patents in Berne. He held this position for eight years. Meantime he married a fellow student, a Serbian girl; he served as an unsalaried lecturer in the University of Berne; and he began publishing his first important papers.

An early believer in Planck's quantum theory (1900), in these early papers Einstein treated problems which invited application of quantum mechanics. In one series (1905–09), on the assumption that propagated radiation has a "quantum-like" structure, he developed the light-quantum hypothesis, and a law of photoelectric effects. He made the first real extension of Planck's fundamental hypothesis in a paper (1907) on the variation of specific heat with temperature. Using the generalized Bohr atom rather than Planck's linear oscillator as

his basic concept, he developed his Law of Radiation. Much earlier he had exhaustively studied the Brownian Movements —those erratic motions of microscopic particles of insoluble matter in still water. Though these movements observably demonstrated the kinetic theory of matter, they had puzzled physicists for eighty years. Now Einstein published a complete theory and working formulas to explain them.

Discussing the Brownian Movements when he was twenty-six, Einstein wrote that "rest and equilibrium can only be an outward semblance which marks a state of disorder and unrest and prepares us for a profound alteration in the aspect of the universe as soon as we alter the scale of our observations. . . . Nature is such that it is impossible to determine absolute motion by any experiment whatever." He was challenging the three-century-long reign of Newton's concept of the universe, signalizing the revolution in scientific thought which has transferred the study of the inner workings of nature from the engineering scientist to the mathematician.

The readers of these early papers recognized in them the scope of imagination and the boldness of deduction of a new master. In 1909 the University of Zurich, where shortly before he had earned his doctoral degree, made him professor extraordinary of theoretical physics; two years later he was named professor of physics at the University of Prague; the next year he returned to Zurich as professor of physics in the Technical Academy; and in 1913, after becoming once more a German citizen, he was named director of the Kaiser-Wilhelm Physical Institute in Berlin. He had now a stipend large enough to allow him to devote all his time, free of routine duties, to research; and he published constantly in the learned journals of Germany, Russia, Switzerland. The Academies of Copenhagen and of Amsterdam and the Royal Society elected him to membership. In 1921, for his work on the photochemical equivalent, the Nobel Prize was conferred upon him.

Six years prior to the award of the Nobel Prize, Einstein had published his generalized theory of relativity, and ten years before that, his restricted theory, with an account of its consequences. The restricted theory had been generally accepted in Germany as early as 1912; but elsewhere it was viewed skeptically. The complete theory made its way slowly, only gradually winning British scientists. By their vote Einstein was awarded the Copley Medal of the Royal Society in 1925, and the Davy Medal in the following year. Both awards were for the relativity theory.

Einstein's growing fame brought him urgent invitations to visit other countries. He had lectured in France in the early 1920s, eager to further friendliness between French and Ger-

man scientists. Now he traveled to India, to China, Japan, Palestine—where he seconded Zionist ambition—to Latin America, England, the United States. Everywhere his vast learning, his modesty, his humanity, his intellectual honesty impressed his hearers. Universities everywhere conferred honorary degrees upon him, and learned societies everywhere pressed him for contributions to their journals. While he was on the Pacific Coast in 1932, Hitler came to power in Germany. When a "German physics" was promulgated, Einstein resigned his directorship of the Institute in Berlin. Almost immediately he became professor of mathematics in the Institute for Advanced Study of Princeton University. In Princeton—an American citizen since 1940—he now resides.

Einstein's theory of relativity grew out of his supposition that the identity in our world of *inertial mass* as measured by Galileo and of *gravitational mass* as measured by Newton is not accidental. If it is not, the Newtonian physics does not explain as wide a range of physical phenomena as is desirable. Classical, or Galilean-Newtonian, physics had explained many natural phenomena in terms of simple forces acting along straight lines, had triumphantly developed astronomy, and, by assuming a mechanical "ether," had applied its principles to problems apparently not mechanical. But the Michelson-Morley experiment on the velocity of light propagation provided sound reasons for denying the existence of an "ether"; the planet Mercury did not behave quite according to the predictions of Newtonian astronomy; electro-magnetic phenomena could not be wholly explained in terms of simple forces. Einstein weighed these difficulties, restudied the fundamental assumptions of physical science, and produced the special theory of relativity.

The special theory makes it possible, by use of the Lorentz transformation, to translate the phenomena of any given inertial system into terms of any other similar system. But Einstein was able to imagine a system not inertial—in fact, to question whether an inertial system could really exist. In 1913, during a walking tour in the Engadine with a party which included Mme. Curie—one of the few mathematicians in Europe sufficiently skilled to discuss his ideas with him—he remarked to her, "What I need to know is what happens to the passengers in an elevator when it falls into emptiness." This problem is not susceptible to experimental solution. But Einstein is a mathematician, not an experimentalist. He did solve the problem, and the answer is the general theory of relativity.

This theory requires that energy and mass being interchangeable and similar in properties, energy—in the form of light, for example—must have weight. It will, therefore, be

deflected in a strong gravitational field. During the eclipse of the sun in 1919, observation startlingly confirmed the theory. Light from the fixed stars was deflected in the neighborhood of the sun, and exactly in the direction and to the amount which Einstein had computed. The theory also satisfactorily explained the aberration in the path of Mercury; using the Maxwell equations, it accounted for the phenomena of electromagnetism. Further, it foretold atomic fission and the transmutation of one element into another, ideas which later skilled experimentation confirmed.

These triumphant demonstrations have led to general acceptance of the theory of relativity, and thus to modern physics. But modern physics differs radically from Newtonian physics. Indeed, it provides a wholly new concept of the physical universe—one in which a mechanical ether does not exist, in which mass and energy are interchangeable, in which absolute rest is impossible, and in which absolute time is unrecognized. Properly the twentieth century may claim to add to the list of builders of world concepts—Pythagoras, Copernicus, Newton—one more: Einstein.

What follows is a condensation of Einstein's *Relativity: The Special and General Theory,* written while he was professor of physics in the University of Berlin.

RELATIVITY

PART ONE: THE SPECIAL THEORY OF RELATIVITY

I. PHYSICAL MEANING OF GEOMETRICAL PROPOSITIONS

GEOMETRY sets out from certain conceptions such as "plane," "point," and "straight line," with which we are able to associate more or less definite ideas, and from certain simple propositions (axioms) which, in virtue of these ideas, we are inclined to accept as "true." Then, on the basis of a logical process, the justification of which we feel ourselves compelled to admit, all remaining propositions are shown to follow from those axioms, *i.e.* they are proven.

If, in pursuance of our habit of thought, we now supplement the propositions of Euclidean geometry by the single proposition that two points on a practically rigid body always correspond to the same distance (line-interval), independently of any changes in position to which we may subject the body, the propositions of Euclidean geometry then resolve themselves into propositions on the possible relative position of practically rigid bodies. Geometry which has been supplemented in this way is then to be treated as a branch of physics. We can now legitimately ask as to the "truth" of geometrical propositions interpreted in this way.

II. THE SYSTEM OF CO-ORDINATES

EVERY DESCRIPTION of the scene of an event or of the position of an object in space is based on the specification of the point on a rigid body (body of reference) with which that event or object coincides. This applies not only to scientific description, but also to everyday life. If I analyse the place specification "Trafalgar Square, London," I arrive at the following result. The earth is the rigid body to which the specification of place refers; "Trafalgar Square, London" is a well-defined point, to which a name has been assigned, and with which the event coincides in space. If a cloud is hovering over Trafalgar Square, then we can determine its position relative to the surface of the earth by erecting a pole perpendicularly on the Square, so that it reaches the cloud. The length of the pole measured with the standard measuring rod, combined with the specifi-

cation of the position of the foot of the pole, supplies us with a complete place specification.

(*a*) We imagine the rigid body, to which the place specification is referred, supplemented in such a manner that the object whose position we require is reached by the completed rigid body.

(*b*) In locating the position of the object, we make use of a number (here the length of the pole measured with the measuring rod) instead of designated points of reference.

(*c*) We speak of the height of the cloud even when the pole which reaches the cloud has not been erected. By means of optical observations of the cloud from different positions on the ground, and taking into account the properties of the propagation of light, we determine the length of the pole we should have required in order to reach the cloud.

From this consideration we see that it will be advantageous if, in the description of position, it should be possible by means of numerical measures to make ourselves independent of the existence of marked positions (possessing names) on the rigid body of reference. In the physics of measurement this is attained by the application of the Cartesian system of co-ordinates.

This consists of three plane surfaces perpendicular to each other and rigidly attached to a rigid body. Referred to a system of co-ordinates, the scene of any event will be determined (for the main part) by the specification of the lengths of the three perpendiculars or co-ordinates (x, y, z) which can be dropped from the scene of the event to those three plane surfaces.

We thus obtain the following result: Every description of events in space involves the use of a rigid body to which such events have to be referred. The resulting relationship takes for granted that the laws of Euclidean geometry hold for "distances," the "distance" being represented physically by means of the convention of two marks on a rigid body.

III. SPACE AND TIME IN CLASSICAL MECHANICS

"THE PURPOSE of mechanics is to describe how bodies change their position in space with time."

It is not clear what is to be understood here by "position" and "space." I stand at the window of a railway carriage which is travelling uniformly, and drop a stone on the embankment, without throwing it. Then, disregarding the influence of the air resistance, I see the stone descend in a straight line. A pedestrian who observes the misdeed from the footpath notices that the stone falls to earth in a parabolic curve. I now ask: Do the "positions" traversed by the stone lie "in reality" on a straight line or on a parabola? Moreover, what is meant here by motion "in space"? From the considerations of the previous chapter the answer is self-evident. In the first place, we entirely shun the vague word "space," of which, we must honestly acknowledge, we cannot form the slightest

conception, and we replace it by "motion relative to a practically rigid body of reference." If instead of "body of reference" we insert "system of co-ordinates," which is a useful idea for mathematical description, we are in a position to say: The stone traverses a straight line relative to a system of co-ordinates rigidly attached to the carriage, but relative to a system of co-ordinates rigidly attached to the ground (embankment) it describes a parabola. With the aid of this example it is clearly seen that there is no such thing as an independently existing trajectory (lit. "path-curve"), but only a trajectory relative to a particular body of reference.

In order to have a *complete* description of the motion, we must specify how the body alters its position *with time; i.e.* for every point on the trajectory it must be stated at what time the body is situated there. These data must be supplemented by such a definition of time that, in virtue of this definition, these time-values can be regarded essentially as magnitudes (results of measurements) capable of observation. If we take our stand on the ground of classical mechanics, we can satisfy this requirement for our illustration in the following manner. We imagine two clocks of identical construction; the man at the railway-carriage window is holding one of them, and the man on the footpath the other. Each of the observers determines the position on his own reference-body occupied by the stone at each tick of the clock he is holding in his hand. In this connection we have not taken account of the inaccuracy involved by the finiteness of the velocity of propagation of light.

IV. THE GALILEIAN SYSTEM OF CO-ORDINATES

As is well known, the fundamental law of the mechanics of Galilei-Newton, which is known as the *law of inertia,* can be stated thus: A body removed sufficiently far from other bodies continues in a state of rest or of uniform motion in a straight line. This law not only says something about the motion of the bodies, but it also indicates the reference-bodies or systems of co-ordinates, permissible in mechanics, which can be used in mechanical description. The visible fixed stars are bodies for which the law of inertia certainly holds to a high degree of approximation. Now if we use a system of co-ordinates which is rigidly attached to the earth, then, relative to this sytem, every fixed star describes a circle of immense radius in the course of an astronomical day, a result which is opposed to the statement of the law of inertia. So that if we adhere to this law we must refer these motions only to systems of co-ordinates relative to which the fixed stars do not move in a circle. A system of co-ordinates of which the state of motion is such that the law of inertia holds relative to it is called a "Galileian system of co-ordinates." The laws of the mechanics of Galilei-Newton can be regarded as valid only for a Galileian system of co-ordinates.

V. THE PRINCIPLE OF RELATIVITY (IN THE RESTRICTED SENSE)

IN ORDER TO ATTAIN the greatest possible clearness, let us return to our example of the railway carriage supposed to be travelling uniformly. We call its motion a uniform translation ("uniform" because it is of constant velocity and direction, "translation" because although the carriage changes its position relative to the embankment yet it does not rotate in so doing). Let us imagine a raven flying through the air in such a manner that its motion, as observed from the embankment, is uniform and in a straight line. If we were to observe the flying raven from the moving railway carriage, we should find that the motion of the raven would be one of different velocity and direction, but that it would still be uniform and in a straight line. Expressed in an abstract manner, we may say: If a mass m is moving uniformly in a straight line with respect to a co-ordinate system K, then it will also be moving uniformly and in a straight line relative to a second co-ordinate system K', provided that the latter is executing a uniform translatory motion with respect to K. In accordance with the discussion contained in the preceding section, it follows that: If, relative to K, K' is a uniformly moving co-ordinate system devoid of rotation, then natural phenomena run their course with respect to K' according to exactly the same general laws as with respect to K. This statement is called the *principle of relativity* (in the restricted sense).

As long as one was convinced that all natural phenomena were capable of representation with the help of classical mechanics, there was no need to doubt the validity of this principle of relativity. But in view of the more recent development of electrodynamics and optics it became more and more evident that classical mechanics affords an insufficient foundation for the physical description of all natural phenomena. At this juncture the question of the validity of the principle of relativity became ripe for discussion.

There are two general facts which at the outset speak very much in favour of the validity of the principle of relativity. It supplies us with the actual motions of the heavenly bodies with a delicacy of detail little short of wonderful. The principle of relativity must therefore apply with great accuracy in the domain of *mechanics*. But that a principle of such broad generality should hold with such exactness in one domain of phenomena, and yet should be invalid for another, is *a priori* not very probable.

We now proceed to the second argument. If the principle of relativity (in the restricted sense) does not hold, we should be constrained to believe that natural laws are capable of being formulated in a particularly simple manner, and of course only on condition that, from amongst all possible Galileian co-ordinate systems, we should have chosen *one* (K_0) of a particular state of motion as our body of reference. We should then be justified in calling this system "absolutely at rest," and all other Galileian

systems K "in motion." If, for instance, our embankment were the system K_0, then our railway carriage would be a system K, relative to which less simple laws would hold than with respect to K_0. This diminished simplicity would be due to the fact that the carriage K would be in motion (*i.e.* "really") with respect to K_0. In the general laws of nature which have been formulated with reference to K, the magnitude and direction of the velocity of the carriage would necessarily play a part. Now in virtue of its motion in an orbit round the sun, our earth is comparable with a railway carriage travelling with a velocity of about 30 kilometres per second. If the principle of relativity were not valid we should therefore expect that the direction of motion of the earth at any moment would enter into the laws of nature, and also that physical systems in their behaviour would be dependent on the orientation in space with respect to the earth. For owing to the alteration in direction of the velocity of revolution of the earth in the course of a year, the earth cannot be at rest relative to the hypothetical system K_0 throughout the whole year. However, the most careful observations have never revealed such anisotropic properties in terrestrial physical space, *i.e.* a physical non-equivalence of different directions. This is a very powerful argument in favour of the principle of relativity.

VI. THE THEOREM OF THE ADDITION OF VELOCITIES EMPLOYED IN CLASSICAL MECHANICS

Let us suppose our old friend the railway carriage to be travelling along the rails with a constant velocity v, and that a man traverses the length of the carriage in the direction of travel with a velocity w. With what velocity W does the man advance relative to the embankment during the process? If the man were to stand still for a second, he would advance relative to the embankment through a distance v equal numerically to the velocity of the carriage. As a consequence of his walking, however, he traverses an additional distance w relative to the carriage, and hence also relative to the embankment, in this second, the distance w being numerically equal to the velocity with which he is walking. Thus in total he covers the distance $W = v + w$ relative to the embankment in the second considered.

VII. THE APPARENT INCOMPATIBILITY OF THE LAW OF PROPAGATION OF LIGHT WITH THE PRINCIPLE OF RELATIVITY

There is hardly a simpler law in physics than that according to which light is propagated in empty space. Every child at school knows, or believes he knows, that this propagation takes place in straight lines with a velocity $c = 300{,}000$ km./sec.

Of course we must refer the process of the propagation of light (and indeed every other process) to a rigid reference-body (co-ordinate system).

As such a system let us again choose our embankment. We shall imagine the air above it to have been removed. If a ray of light be sent along the embankment, we see from the above that the tip of the ray will be transmitted with the velocity c relative to the embankment. Now let us suppose that our railway carriage is again travelling along the railway lines with the velocity v, and that its direction is the same as that of the ray of light, but its velocity of course much less. It is obvious that we can here apply the consideration of the previous section, since the ray of light plays the part of the man walking along relatively to the carriage. w is the required velocity of light with respect to the carriage, and we have

$$w = c - v.$$

The velocity of propagation of a ray of light relative to the carriage thus comes out smaller than c.

But this result comes into conflict with the principle of relativity set forth in Chapter V. For, like every other general law of nature, the law of the transmission of light *in vacuo* must, according to the principle of relativity, be the same for the railway carriage as reference-body as when the rails are the body of reference. But if every ray of light is propagated relative to the embankment with the velocity c, then for this reason it would appear that another law of propagation of light must necessarily hold with respect to the carriage—a result contradictory to the principle of relativity.

In view of this dilemma there appears to be nothing else for it than to abandon either the principle of relativity or the simple law of the propagation of light *in vacuo*. The epoch-making theoretical investigations of H. A. Lorentz on the electrodynamical and optical phenomena connected with moving bodies lead conclusively to a theory of electromagnetic phenomena, of which the law of the constancy of the velocity of light *in vacuo* is a necessary consequence. Prominent theoretical physicists were therefore more inclined to reject the principle of relativity.

At this juncture the theory of relativity entered the arena. As a result of an analysis of the physical conceptions of time and space, it became evident that *in reality there is not the least incompatibility between the principle of relativity and the law of propagation of light,* and that by systematically holding fast to both these laws a logically rigid theory could be arrived at. This theory has been called the *special theory of relativity.*

VIII. ON THE IDEA OF TIME IN PHYSICS

LIGHTNING has struck the rails on our railway embankment at two places A and B far distant from each other. I make the additional assertion that these two lightning flashes occurred simultaneously. If I now approach you with the request to explain to me the sense of the statement more precisely, you find after some consideration that the answer to this question is not so easy as it appears at first sight.

After thinking the matter over for some time you offer the following suggestion with which to test simultaneity. By measuring along the rails, the connecting line AB should be measured and an observer placed at the mid-point M of the distance AB. This observer should be supplied with an arrangement (*e.g.* two mirrors inclined at 90°) which allows him visually to observe both places A and B at the same time. If the observer perceives the two flashes of lightning at the same time, then they are simultaneous.

I am very pleased with this suggestion. You declare: "There is only *one* demand to be made of the definition of simultaneity, namely, that in every real case it must supply us with an empirical decision as to whether or not the conception that has to be defined is fulfilled. That light requires the same time to traverse the path $A \longrightarrow M$ as for the path $B \longrightarrow M$ is in reality neither a *supposition nor a hypothesis* about the physical nature of light, but a *stipulation.*"

It is clear that this definition can be used to give an exact meaning not only to *two* events, but to as many events as we care to choose, and independently of the positions of the scenes of the events with respect to the body of reference (here the railway embankment). We are thus led also to a definition of "time" in physics. For this purpose we suppose that clocks of identical construction are placed at the points A, B and C of the railway line (co-ordinate system), and that they are set in such a manner that the positions of their pointers are simultaneously (in the above sense) the same. Under these conditions we understand by the "time" of an event the reading (position of the hands) of that one of these clocks which is in the immediate vicinity (in space) of the event. In this manner a time-value is associated with every event which is essentially capable of observation.

IX. THE RELATIVITY OF SIMULTANEITY

We suppose a very long train travelling along the rails with the constant velocity v and in the direction indicated in Fig. 1. People travelling in this train will with advantage use the train as a rigid reference-body (co-

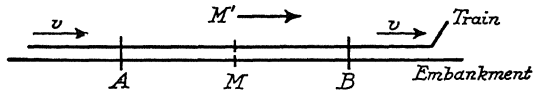

Fig. 1.

ordinate system); they regard all events in reference to the train. Then every event which takes place along the line also takes place at a particular point of the train.

Are two events (*e.g.* the two strokes of lightning A and B) which are simultaneous *with reference to the railway embankment* also simultaneous *relatively to the train?*

When we say that the lightning strokes A and B are simultaneous with respect to the embankment, we mean: the rays of light emitted at the places A and B, where the lightning occurs, meet each other at the mid-point M of the length $A \longrightarrow B$ of the embankment. But the events A and B also correspond to positions A and B on the train. Let M' be the mid-point of the distance $A \longrightarrow B$ on the travelling train. Just when the flashes of lightning occur, this point M' naturally coincides with the point M, but it moves towards the right in the diagram with the velocity v of the train. If an observer sitting in the position M' in the train did not possess this velocity, then he would remain permanently at M, and the light rays emitted by the flashes of lightning A and B would reach him simultaneously, *i.e.* they would meet just where he is situated. Now in reality (considered with reference to the railway embankment) he is hastening towards the beam of light coming from B, whilst he is riding on ahead of the beam of light coming from A. Hence the observer will see the beam of light emitted from B earlier than he will see that emitted from A. Observers who take the railway train as their reference-body must therefore come to the conclusion that the lightning flash B took place earlier than the lightning flash A. We thus arrive at the important result:

Events which are simultaneous with reference to the embankment are not simultaneous with respect to the train, and *vice versa* (relativity of simultaneity). Every reference-body (co-ordinate system) has its own particular time; unless we are told the reference-body to which the statement of time refers, there is no meaning in a statement of the time of an event.

We concluded that the man in the carriage, who traverses the distance *w per second* relative to the carriage, traverses the same distance also with respect to the embankment *in each second* of time. But, according to the foregoing considerations, the time required by a particular occurrence with respect to the carriage must not be considered equal to the duration of the same occurrence as judged from the embankment (as reference-body). Hence it cannot be contended that the man in walking travels the distance w relative to the railway line in a time which is equal to one second as judged from the embankment.

X. ON THE RELATIVITY OF THE CONCEPTION OF DISTANCE

Let us consider two particular points on the train travelling along the embankment with the velocity v, and inquire as to their distance apart. It is the simplest plan to use the train itself as the reference-body (co-ordinate system). An observer in the train measures the interval by marking off his measuring rod in a straight line (*e.g.* along the floor of the carriage) as many times as is necessary to take him from the one marked point to the other.

It is a different matter when the distance has to be judged from the

railway line. If we call A' and B' the two points on the train whose distance apart is required, then both of these points are moving with the velocity v along the embankment. In the first place we require to determine the points A and B of the embankment which are just being passed by the two points A' and B' at a particular time t—judged from the embankment. These points A and B of the embankment can be determined by applying the definition of time given in Chapter VIII. The distance between these points A and B is then measured by repeated application of the measuring rod along the embankment.

A priori it is by no means certain that this last measurement will supply us with the same result as the first. Thus the length of the train as measured from the embankment may be different from that obtained by measuring in the train itself. This circumstance leads us to a second objection which must be raised against the apparently obvious consideration of Chapter VI. Namely, if the man in the carriage covers the distance w in a unit of time—*measured from the train*—then this distance—*as measured from the embankment*—is not necessarily also equal to w.

XI. THE LORENTZ TRANSFORMATION

THE RESULTS of the last three chapters show that the apparent incompatibility of the law of propagation of light with the principle of relativity (Chapter VII) has been derived by means of a consideration which borrowed two unjustifiable hypotheses from classical mechanics; these are as follows:

(1) The time-interval (time) between two events is independent of the condition of motion of the body of reference.
(2) The space-interval (distance) between two points of a rigid body is independent of the condition of motion of the body of reference.

If we drop these hypotheses, then the dilemma of Chapter VII disappears, because the theorem of the addition of velocities derived in Chapter VI becomes invalid. The possibility presents itself that the law of the propagation of light *in vacuo* may be compatible with the principle of relativity. In the discussion of Chapter VI we have to do with places and times relative both to the train and to the embankment. Can we conceive of a relation between place and time of the individual events relative to both reference-bodies, such that every ray of light possesses the velocity of transmission c relative to the embankment and relative to the train?

Up to the present we have only considered events taking place along the embankment, which had mathematically to assume the function of a straight line. In the manner indicated in Chapter II we can imagine this reference-body supplemented laterally and in a vertical direction by means of a framework of rods, so that an event which takes place any-

where can be localised with reference to this framework. Similarly, we can imagine the train travelling with the velocity v to be continued across the whole of space, so that every event, no matter how far off it may be, could also be localised with respect to the second framework. In every such framework we imagine three surfaces perpendicular to each other marked out, and designated as "co-ordinate planes" ("co-ordinate system"). A co-ordinate system K then corresponds to the embankment, and a co-ordinate system K' to the train. An event, wherever it may have taken place, would be fixed in space with respect to K by the three perpendiculars x, y, z on the co-ordinate planes, and with regard to time by a time-value t. Relative to K', *the same event* would be fixed in respect of space and time by corresponding values x', y', z', t', which of course are not identical with x, y, z, t.

What are the values x', y', z', t' of an event with respect to K', when the magnitudes x, y, z, t of the same event with respect to K are given? The relations must be so chosen that the law of the transmission of light

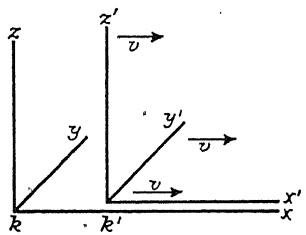

FIG. 2.

in vacuo is satisfied for one and the same ray of light (and of course for every ray) with respect to K and K'. For the relative orientation in space of the co-ordinate systems indicated in the diagram (Fig. 2), this problem is solved by means of the equations:

$$x' = \frac{x - vt}{\sqrt{1 - \frac{v^2}{c^2}}}$$

$$y' = y$$

$$z' = z$$

$$t' = \frac{t - \frac{v}{c^2} \cdot x}{\sqrt{1 - \frac{v^2}{c^2}}}$$

This system of equations is known as the "Lorentz transformation."

If in place of the law of transmission of light we had taken as our basis the tacit assumptions of the older mechanics as to the absolute

character of times and lengths, then instead of the above we should have obtained the following equations:

$$x' = x - vt$$
$$y' = y$$
$$z' = z$$
$$t' = t.$$

This system of equations is often termed the "Galilei transformation." The Galilei transformation can be obtained from the Lorentz transformation by substituting an infinitely large value for the velocity of light c in the latter transformation.

XII. THE BEHAVIOUR OF MEASURING RODS AND CLOCKS IN MOTION

I PLACE a metre-rod in the x'-axis of K' in such a manner that one end (the beginning) coincides with the point $x' = 0$, whilst the other end (the end of the rod) coincides with the point $x' = 1$. What is the length of the metre-rod relatively to the system K? In order to learn this, we need only ask where the beginning of the rod and the end of the rod lie with respect to K at a particular time t of the system K. By means of the first equation of the Lorentz transformation the values of these two points at the time $t = 0$ can be shown to be

$$x(\text{beginning of rod}) = 0 . \sqrt{1 - \frac{v^2}{c^2}}$$

$$x(\text{end of rod}) = 1 . \sqrt{1 - \frac{v^2}{c^2}},$$

the distance between the points being $\sqrt{1 - \frac{v^2}{c^2}}$. But the metre-rod is moving with the velocity v relative to K. It therefore follows that the length of a rigid metre-rod moving in the direction of its length with a velocity v is $\sqrt{1 - v^2/c^2}$ of a metre. The rigid rod is thus shorter when in motion than when at rest, and the more quickly it is moving, the shorter is the rod. For the velocity $v = c$ we should have $\sqrt{1 - v^2/c^2} = 0$, and for still greater velocities the square root becomes imaginary. From this we conclude that in the theory of relativity the velocity c plays the part of a limiting velocity, which can neither be reached nor exceeded by any real body.

If, on the contrary, we had considered a metre-rod at rest in the x-axis with respect to K, then we should have found that the length of the rod as judged from K' would have been $\sqrt{1 - v^2/c^2}$; this is quite in accordance with the principle of relativity which forms the basis of our

considerations. If we had based our considerations on the Galilei transformation we should not have obtained a contraction of the rod as a consequence of its motion.

Let us now consider a seconds clock which is permanently situated at the origin ($x' = 0$) of K'; $t' = 0$ and $t' = 1$ are two successive ticks of this clock. The first and fourth equations of the Lorentz transformation give for these two ticks:

$$t = 0$$

and

$$t = \frac{1}{\sqrt{1 - \frac{v^2}{c^2}}}.$$

As judged from K, the clock is moving with the velocity v; as judged from this reference-body, the time which elapses between two strokes of the clock is not one second, but $\frac{1}{\sqrt{1 - \frac{v^2}{c^2}}}$ seconds, *i. e.* a somewhat larger time. As a consequence of its motion the clock goes more slowly than when at rest. Here also the velocity c plays the part of an unattainable limiting velocity.

XIII. THEOREM OF THE ADDITION OF VELOCITIES. THE EXPERIMENT OF FIZEAU

In Chapter VI we derived the theorem of the addition of velocities in one direction in the form which also results from the hypotheses of classical mechanics. This theorem can also be deduced readily from the Galilei transformation (Chapter XI). In place of the man walking inside the carriage, we introduce a point moving relatively to the co-ordinate system K' in accordance with the equation

$$x' = wt'.$$

By means of the first and fourth equations of the Galilei transformation we can express x' and t' in terms of x and t, and we then obtain

$$x = (v + w)t.$$

This equation expresses nothing else than the law of motion of the point with reference to the system K (of the man with reference to the embankment). We denote this velocity by the symbol W, and we then obtain, as in Chapter VI,

$$W = v + w \quad \text{.....(A).}$$

But we can carry out this consideration just as well on the basis of the theory of relativity. In the equation

$$x' = wt'$$

we must then express x' and t' in terms of x and t, making use of the first and fourth equations of the *Lorentz transformation*. Instead of the equation (A) we then obtain the equation

$$W = \frac{v + w}{1 + \frac{vw}{c^2}} \quad \text{(B)},$$

which corresponds to the theorem of addition for velocities in one direction according to the theory of relativity. The question now arises as to which of these two theorems is the better in accord with experience. On this point we are enlightened by a most important experiment which the brilliant physicist Fizeau performed more than half a century ago.

The experiment is concerned with the following question. Light travels in a motionless liquid with a particular velocity w. How quickly does it travel in the direction of the arrow in the tube T (see the accompanying diagram, Fig. 3) when the liquid above mentioned is flowing through the tube with a velocity v?

In accordance with the principle of relativity we shall certainly have to take for granted that the propagation of light always takes place with the same velocity w *with respect to the liquid*, whether the latter is in motion with reference to other bodies or not. The velocity of light relative to the liquid and the velocity of the latter relative to the tube are thus known, and we require the velocity of light relative to the tube.

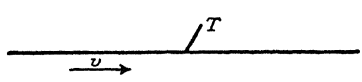

FIG. 3.

If we denote the velocity of the light relative to the tube by W, then this is given by the equation (A) or (B), according as the Galilei transformation or the Lorentz transformation corresponds to the facts. Experiment decides in favour of equation (B) derived from the theory of relativity, and the agreement is, indeed, very exact.

XIV. THE HEURISTIC VALUE OF THE THEORY OF RELATIVITY

OUR TRAIN OF THOUGHT in the foregoing pages can be epitomised in the following manner.

Every general law of nature must be so constituted that it is transformed into a law of exactly the same form when, instead of the space-time variables x, y, z, t of the original co-ordinate system K, we introduce new space-time variables x', y', z', t' of a co-ordinate system K'. In this connection the relation between the ordinary and the accented magnitudes is given by the Lorentz transformation. Or, in brief: General laws of nature are co-variant with respect to Lorentz transformations.

This is a definite mathematical condition that the theory of relativity demands of a natural law, and in virtue of this, the theory becomes a valuable heuristic aid in the search for general laws of nature. If a general law of nature were to be found which did not satisfy this condition, then at least one of the two fundamental assumptions of the theory would have been disproved. Let us now examine what general results the latter theory has hitherto evinced.

XV. GENERAL RESULTS OF THE THEORY

IT IS CLEAR from our previous considerations that the (special) theory of relativity has grown out of electrodynamics and optics. In these fields it has not appreciably altered the predictions of theory, but it has considerably simplified the theoretical structure, *i.e.* the derivation of laws, and—what is incomparably more important—it has considerably reduced the number of independent hypotheses forming the basis of theory.

Classical mechanics required to be modified before it could come into line with the demands of the special theory of relativity. For the main part, however, this modification affects only the laws for rapid motions, in which the velocities of matter v are not very small as compared with the velocity of light. We have experience of such rapid motions only in the case of electrons and ions; for other motions the variations from the laws of classical mechanics are too small to make themselves evident in practice. In accordance with the theory of relativity the kinetic energy of a material point of mass m is no longer given by the well-known expression

$$m\frac{v^2}{2},$$

but by the expression

$$\frac{mc^2}{\sqrt{1-\frac{v^2}{c^2}}}.$$

This expression approaches infinity as the velocity v approaches the velocity of light c. The velocity must therefore always remain less than c, however great may be the energies used to produce the acceleration. If we

develop the expression for the kinetic energy in the form of a series, we obtain

$$mc^2 + m\frac{v^2}{2} + \frac{3}{8}m\frac{v^4}{c^2} + \cdots$$

When $\frac{v^2}{c^2}$ is small compared with unity, the third of these terms is always small in comparison with the second, which last is alone considered in classical mechanics. The first term mc^2 does not contain the velocity, and requires no consideration if we are only dealing with the question as to how the energy of a point-mass depends on the velocity.

Before the advent of relativity, physics recognised two conservation laws of fundamental importance, namely, the law of the conservation of energy and the law of the conservation of mass; these two fundamental laws appeared to be quite independent of each other. By means of the theory of relativity they have been united into one law.

The principle of relativity requires that the law of the conservation of energy should hold not only with reference to a co-ordinate system K, but also with respect to every co-ordinate system K' which is in a state of uniform motion of translation relative to K, or, briefly, relative to every "Galilean" system of co-ordinates. In contrast to classical mechanics, the Lorentz transformation is the deciding factor in the transition from one such system to another.

By means of comparatively simple considerations we are led to draw the following conclusion from these premises, in conjunction with the fundamental equations of the electrodynamics of Maxwell: A body moving with the velocity v, which absorbs an amount of energy E_0 in the form of radiation without suffering an alteration in velocity in the process, has, as a consequence, its energy increased by an amount

$$\frac{E_0}{\sqrt{1-\frac{v^2}{c^2}}}.$$

In consideration of the expression given above for the kinetic energy of the body, the required energy of the body comes out to be

$$\frac{\left(m+\frac{E_0}{c^2}\right)c^2}{\sqrt{1-\frac{v^2}{c^2}}}.$$

Thus the body has the same energy as a body of mass $\left(m+\frac{E_0}{c^2}\right)$ moving with the velocity v. Hence we can say: If a body takes up an amount of energy E_0, then its inertial mass increases by an amount $\frac{E_0}{c^2}$; the inertial mass of a body is not a constant, but varies according to the

change in the energy of the body. The inertial mass of a system of bodies can even be regarded as a measure of its energy. The law of the conservation of the mass of a system becomes identical with the law of the conservation of energy, and is only valid provided that the system neither takes up nor sends out energy. Writing the expression for the energy in the form

$$\frac{mc^2 + E_0}{\sqrt{1 - \frac{v^2}{c^2}}},$$

we see that the term mc^2 is nothing else than the energy possessed by the body before it absorbed the energy E_0.

A direct comparison of this relation with experiment is not possible at the present time, owing to the fact that the changes in energy E_0 to which we can subject a system are not large enough to make themselves perceptible as a change in the inertial mass of the system. $\frac{E_0}{c^2}$ is too small in comparison with the mass m, which was present before the alteration of the energy. It is owing to this circumstance that classical mechanics was able to establish successfully the conservation of mass as a law of independent validity.

XVI. EXPERIENCE AND THE SPECIAL THEORY OF RELATIVITY

IT IS KNOWN that cathode rays and the so-called β-rays emitted by radioactive substances consist of negatively electrified particles (electrons) of very small inertia and large velocity. By examining the deflection of these rays under the influence of electric and magnetic fields, we can study the law of motion of these particles very exactly.

In the theoretical treatment of these electrons, we are faced with the difficulty that electrodynamic theory of itself is unable to give an account of their nature. For since electrical masses of one sign repel each other, the negative electrical masses constituting the electron would necessarily be scattered under the influence of their mutual repulsions, unless there are forces of another kind operating between them, the nature of which has hitherto remained obscure to us. If we now assume that the relative distances between the electrical masses constituting the electron remain unchanged during the motion of the electron (rigid connection in the sense of classical mechanics), we arrive at a law of motion of the electron which does not agree with experience. Guided by purely formal points of view, H. A. Lorentz was the first to introduce the hypothesis that the particles constituting the electron experience a contraction in the direction of motion in consequence of that motion, the amount of this contraction being proportional to the expression $\sqrt{1 - \frac{v^2}{c^2}}$. This hypothesis,

which is not justifiable by any electrodynamical facts, supplies us then with that particular law of motion which has been confirmed with great precision in recent years.

The theory of relativity leads to the same law of motion, without requiring any special hypothesis whatsoever as to the structure and the behaviour of the electron.

XVII. MINKOWSKI'S FOUR-DIMENSIONAL SPACE

SPACE is a three-dimensional continuum. By this we mean that it is possible to describe the position of a point (at rest) by means of three numbers (co-ordinates) x, y, z, and that there is an indefinite number of points in the neighbourhood of this one, the position of which can be described by co-ordinates such as x_1, y_1, z_1, which may be as near as we choose to the respective values of the co-ordinates x, y, z of the first point. In virtue of the latter property we speak of a "continuum," and owing to the fact that there are three co-ordinates we speak of it as being "three-dimensional."

Similarly, the world of physical phenomena which was briefly called "world" by Minkowski is naturally four-dimensional in the space-time sense. For it is composed of individual events, each of which is described by four numbers, namely, three space co-ordinates x, y, z and a time co-ordinate, the time-value t. The "world" is in this sense also a continuum; for to every event there are as many "neighbouring" events (realised or at least thinkable) as we care to choose, the co-ordinates x_1, y_1, z_1, t_1 of which differ by an indefinitely small amount from those of the event x, y, z, t originally considered. As a matter of fact, according to classical mechanics, time is absolute, *i.e.* it is independent of the position and the condition of motion of the system of co-ordinates. We see this expressed in the last equation of the Galileian transformation ($t' = t$).

The four-dimensional mode of consideration of the "world" is natural on the theory of relativity, since according to this theory time is robbed of its independence. But the discovery of Minkowski, which was of importance for the formal development of the theory of relativity, does not lie here. It is to be found rather in the fact of his recognition that the four-dimensional space-time continuum of the theory of relativity, in its most essential formal properties, shows a pronounced relationship to the three-dimensional continuum of Euclidean geometrical space. In order to give due prominence to this relationship, however, we must replace the usual time co-ordinate t by an imaginary magnitude $\sqrt{-1}.ct$ proportional to it. Under these conditions, the natural laws satisfying the demands of the (special) theory of relativity assume mathematical forms, in which the time co-ordinate plays exactly the same role as the three space co-ordinates. Formally, these four co-ordinates correspond exactly to the three space co-ordinates in Euclidean geometry. It must be clear even to the non-mathematician that, as a consequence of this purely

formal addition to our knowledge, the theory perforce gained clearness in no mean measure.

PART TWO: THE GENERAL THEORY OF RELATIVITY

XVIII. SPECIAL AND GENERAL PRINCIPLE OF RELATIVITY

THE BASAL PRINCIPLE, which was the pivot of all our previous considerations, was the *special* principle of relativity, *i.e.* the principle of the physical relativity of all *uniform* motion.

The principle we have made use of not only maintains that we may equally well choose the carriage or the embankment as our reference-body for the description of any event. Our principle rather asserts what follows: If we formulate the general laws of nature as they are obtained from experience, by making use of

(*a*) the embankment as reference-body,

(*b*) the railway carriage as reference-body,

then these general laws of nature (*e.g.* the laws of mechanics or the law of the propagation of light *in vacuo*) have exactly the same form in both cases. This can also be expressed as follows: For the *physical* description of natural processes, neither of the reference-bodies K, K' is unique (lit. "specially marked out") as compared with the other. Unlike the first, this latter statement need not of necessity hold *a priori;* it is not contained in the conceptions of "motion" and "reference-body" and derivable from them; only *experience* can decide as to its correctness or incorrectness.

We started out from the assumption that there exists a reference-body K, whose condition of motion is such that the Galileian law holds with respect to it: A particle left to itself and sufficiently far removed from all other particles moves uniformly in a straight line. With reference to K (Galileian reference-body) the laws of nature were to be as simple as possible. But in addition to K, all bodies of reference K' should be given preference in this sense, and they should be exactly equivalent to K for the formulation of natural laws, provided that they are in a state of *uniform rectilinear and non-rotary motion* with respect to K; all these bodies of reference are to be regarded as Galileian reference-bodies. The validity of the principle of relativity was assumed only for these reference-bodies, but not for others (*e.g.* those possessing motion of a different kind). In this sense we speak of the *special* principle of relativity, or special theory of relativity.

In contrast to this we wish to understand by the "general principle of relativity" the following statement: All bodies of reference K, K', etc., are equivalent for the description of natural phenomena (formulation of the general laws of nature), whatever may be their state of motion.

Let us imagine ourselves transferred to our old friend the railway carriage, which is travelling at a uniform rate. As long as it is moving uniformly, the occupant of the carriage is not sensible of its motion, and it

is for this reason that he can without reluctance interpret the facts of the case as indicating that the carriage is at rest, but the embankment in motion. Moreover, according to the special principle of relativity, this interpretation is quite justified also from a physical point of view.

If the motion of the carriage is now changed into a non-uniform motion, as for instance by a powerful application of the brakes, then the occupant of the carriage experiences a correspondingly powerful jerk forwards. It is clear that the Galileian law does not hold with respect to the non-uniformly moving carriage. Because of this, we feel compelled at the present juncture to grant a kind of absolute physical reality to non-uniform motion, in opposition to the general principle of relativity.

XIX. THE GRAVITATIONAL FIELD

"IF WE PICK UP a stone and then let it go, why does it fall to the ground?" The usual answer to this question is: "Because it is attracted by the earth." Modern physics formulates the answer rather differently for the following reason. As a result of the more careful study of electromagnetic phenomena, we have come to regard action at a distance as a process impossible without the intervention of some intermediary medium. If, for instance, a magnet attracts a piece of iron, we cannot be content to regard this as meaning that the magnet acts directly on the iron through the intermediate empty space, but we are constrained to imagine—after the manner of Faraday—that the magnet always calls into being something physically real in the space around it, that something being what we call a "magnetic field." In its turn this magnetic field operates on the piece of iron, so that the latter strives to move towards the magnet.

The action of the earth on the stone takes place indirectly. The earth produces in its surroundings a gravitational field, which acts on the stone and produces its motion of fall. As we know from experience, the intensity of the action on a body diminishes according to a quite definite law, as we proceed farther and farther away from the earth. From our point of view this means: The body (*e.g.* the earth) produces a field in its immediate neighbourhood directly; the intensity and direction of the field at points farther removed from the body are thence determined by the law which governs the properties in space of the gravitational fields themselves.

In contrast to electric and magnetic fields, the gravitational field exhibits a most remarkable property, which is of fundamental importance for what follows. Bodies which are moving under the sole influence of a gravitational field receive an acceleration, *which does not in the least depend either on the material or on the physical state of the body.* This law, which holds most accurately, can be expressed in a different form in the light of the following consideration.

According to Newton's law of motion, we have

$$(\text{Force}) = (\text{inertial mass}) \times (\text{acceleration}),$$

where the "inertial mass" is a characteristic constant of the accelerated body. If now gravitation is the cause of the acceleration, we then have

$$(\text{Force}) = (\text{gravitational mass}) \times (\text{intensity of the gravitational field}),$$

where the "gravitational mass" is likewise a characteristic constant for the body. From these two relations follows:

$$(\text{acceleration}) = \frac{(\text{gravitational mass})}{(\text{inertial mass})} \times (\text{intensity of the gravitational field}).$$

If now, as we find from experience, the acceleration is to be independent of the nature and the condition of the body and always the same for a given gravitational field, then the ratio of the gravitational to the inertial mass must likewise be the same for all bodies. By a suitable choice of units we can thus make this ratio equal to unity. We then have the following law: The *gravitational* mass of a body is equal to its *inertial* mass.

It is true that this important law had hitherto been recorded in mechanics, but it had not been *interpreted*. A satisfactory interpretation can be obtained only if we recognise the following fact: *The same* quality of a body manifests itself according to circumstances as "inertia" or as "weight" (lit. "heaviness").

XX. THE EQUALITY OF INERTIAL AND GRAVITATIONAL MASS AS AN ARGUMENT FOR THE GENERAL POSTULATE OF RELATIVITY

WE IMAGINE a large portion of empty space, so far removed from stars and other appreciable masses that we have before us approximately the conditions required by the fundamental law of Galilei. As reference-body let us imagine a spacious chest resembling a room with an observer inside who is equipped with apparatus. Gravitation naturally does not exist for this observer. He must fasten himself with strings to the floor, otherwise the slightest impact against the floor will cause him to rise slowly towards the ceiling of the room.

To the middle of the lid of the chest is fixed externally a hook with rope attached, and now a "being" (what kind of a being is immaterial to us) begins pulling at this with a constant force. The chest together with the observer then begins to move "upwards" with a uniformly accelerated motion. In course of time its velocity will reach unheard-of values—provided that we are viewing all this from another reference-body which is not being pulled with a rope.

But how does the man in the chest regard the process? The acceleration of the chest will be transmitted to him by the reaction of the floor of the chest. If he release a body which he previously had in his hand,

the acceleration of the chest will no longer be transmitted to this body, and for this reason the body will approach the floor of the chest with an accelerated relative motion. The observer will further convince himself *that the acceleration of the body towards the floor of the chest is always of the same magnitude, whatever kind of body he may happen to use for the experiment.*

Relying on his knowledge of the gravitational field (as it was discussed in the preceding chapter), the man in the chest will thus come to the conclusion that he and the chest are in a gravitational field which is constant with regard to time. Of course he will be puzzled for a moment as to why the chest does not fall in this gravitational field. Just then, however, he discovers the hook in the middle of the lid of the chest and the rope which is attached to it, and he consequently comes to the conclusion that the chest is suspended at rest in the gravitational field.

Even though it is being accelerated with respect to the "Galileian space" first considered, we can nevertheless regard the chest as being at rest. We have thus good grounds for extending the principle of relativity to include bodies of reference which are accelerated with respect to each other, and as a result we have gained a powerful argument for a generalised postulate of relativity.

Suppose that the man in the chest fixes a rope to the inner side of the lid, and that he attaches a body to the free end of the rope. The result of this will be to stretch the rope so that it will hang "vertically" downwards. If we ask for an opinion of the cause of tension in the rope, the man in the chest will say: "The suspended body experiences a downward force in the gravitational field, and this is neutralised by the tension of the rope; what determines the magnitude of the tension of the rope is the *gravitational mass* of the suspended body." On the other hand, an observer who is poised freely in space will interpret the condition of things thus: "The rope must perforce take part in the accelerated motion of the chest, and it transmits this motion to the body attached to it. The tension of the rope is just large enough to effect the acceleration of the body. That which determines the magnitude of the tension of the rope is the *inertial mass* of the body." Guided by this example, we see that our extension of the principle of relativity implies the *necessity* of the law of the equality of inertial and gravitational mass. Thus we have obtained a physical interpretation of this law.

We can now appreciate why that argument is not convincing which we brought forward against the general principle of relativity at the end of Chapter XVIII. It is certainly true that the observer in the railway carriage experiences a jerk forwards as a result of the application of the brake, and that he recognises in this the non-uniformity of motion (retardation) of the carriage. But he is compelled by nobody to refer this jerk to a "real" acceleration (retardation) of the carriage. He might also interpret his experience thus: "My body of reference (the carriage) remains permanently at rest. With reference to it, however, there exists (during the period of application of the brakes) a gravitational field

which is directed forwards and which is variable with respect to time. Under the influence of this field, the embankment together with the earth moves non-uniformly in such a manner that their original velocity in the backwards direction is continuously reduced."

XXI. IN WHAT RESPECTS ARE THE FOUNDATIONS OF CLASSICAL MECHANICS AND OF THE SPECIAL THEORY OF RELATIVITY UNSATISFACTORY?

WE HAVE ALREADY STATED several times that classical mechanics starts out from the following law: Material particles sufficiently far removed from other material particles continue to move uniformly in a straight line or continue in a state of rest. We have also repeatedly emphasised that this fundamental law can only be valid for bodies of reference K which possess certain unique states of motion, and which are in uniform translational motion relative to each other. Relative to other reference-bodies K the law is not valid. Both in classical mechanics and in the special theory of relativity we therefore differentiate between reference-bodies K relative to which the recognised "laws of nature" can be said to hold and reference-bodies K relative to which these laws do not hold.

But no person whose mode of thought is logical can rest satisfied with this condition of things. He asks: "How does it come that certain reference-bodies (or their states of motion) are given priority over other reference-bodies (or their states of motion)? *What is the reason for this preference?*"

I seek in vain for a real something in classical mechanics (or in the special theory of relativity) to which I can attribute the different behaviour of bodies considered with respect to the reference-systems K and K'. Newton saw this objection and attempted to invalidate it, but without success. It can only be got rid of by means of a physics which is conformable to the general principle of relativity, since the equations of such a theory hold for every body of reference, whatever may be its state of motion.

XXII. A FEW INFERENCES FROM THE GENERAL THEORY OF RELATIVITY

THE CONSIDERATIONS of Chapter XX show that the general theory of relativity puts us in a position to derive properties of the gravitational field in a purely theoretical manner. Let us suppose, for instance, that we know the space-time "course" for any natural process whatsoever, as regards the manner in which it takes place in the Galileian domain relative to a Galileian body of reference K. By means of purely theoretical operations (*i.e.* simply by calculation) we are then able to find how this known natural process appears, as seen from a reference-body K' which is accelerated

relatively to K. But since a gravitational field exists with respect to this new body of reference K', our consideration also teaches us how the gravitational field influences the process studied.

For example, we learn that a body which is in a state of uniform rectilinear motion with respect to K (in accordance with the law of Galilei) is executing an accelerated and in general curvilinear motion with respect to the accelerated reference-body K' (chest). This acceleration or curvature corresponds to the influence on the moving body of the gravitational field prevailing relatively to K'. It is known that a gravitational field influences the movement of bodies in this way, so that our consideration supplies us with nothing essentially new.

However, we obtain a new result of fundamental importance when we carry out the analogous consideration for a ray of light. With respect to the Galileian reference-body K, such a ray of light is transmitted rectilinearly with the velocity c. It can easily be shown that the path of the same ray of light is no longer a straight line when we consider it with reference to the accelerated chest (reference-body K'). From this we conclude *that, in general, rays of light are propagated curvilinearly in gravitational fields.*

Although a detailed examination of the question shows that the curvature of light rays required by the general theory of relativity is only exceedingly small for the gravitational fields at our disposal in practice, its estimated magnitude for light rays passing the sun at grazing incidence is nevertheless 1.7 seconds of arc. This ought to manifest itself in the following way: As seen from the earth, certain fixed stars appear to be in the neighbourhood of the sun, and are thus capable of observation during a total eclipse of the sun. At such times, these stars ought to appear to be displaced outwards from the sun by an amount indicated above, as compared with their apparent position in the sky when the sun is situated at another part of the heavens. The examination of the correctness or otherwise of this deduction is a problem of the greatest importance, the early solution of which is to be expected of astronomers.[1]

In the second place our result shows that, according to the general theory of relativity, the law of the constancy of the velocity of light *in vacuo,* which constitutes one of the two fundamental assumptions in the special theory of relativity and to which we have already frequently referred, cannot claim any unlimited validity. A curvature of rays of light can only take place when the velocity of propagation of light varies with position. Now we might think that as a consequence of this, the special theory of relativity and with it the whole theory of relativity would be laid in the dust. But in reality this is not the case. We can only conclude that the special theory of relativity cannot claim an unlimited domain of validity; its results hold only so long as we are able to disregard the influences of gravitational fields on the phenomena (*e.g.* of light).

[1]By means of the star photographs of two expeditions equipped by a Joint Committee of the Royal and Royal Astronomical Societies, the existence of the deflection of light demanded by theory was confirmed during the solar eclipse of May 29, 1919.

The most attractive problem, to the solution of which the general theory of relativity supplies the key, concerns the investigation of the laws satisfied by the gravitational field itself. Let us consider this for a moment.

We are acquainted with space-time domains which behave (approximately) in a "Galileian" fashion under suitable choice of reference-body, *i.e.* domains in which gravitational fields are absent. If we now refer such a domain to a reference-body K' possessing any kind of motion, then relative to K' there exists a gravitational field which is variable with respect to space and time. According to the general theory of relativity, the general law of the gravitational field must be satisfied for all gravitational fields obtainable in this way.

XXIII. BEHAVIOUR OF CLOCKS AND MEASURING RODS ON A ROTATING BODY OF REFERENCE

We start off again from quite special cases, which we have frequently used before. Let us consider a space-time domain in which no gravitational field exists relative to a reference-body K whose state of motion has been suitably chosen. K is then a Galileian reference-body as regards the domain considered, and the results of the special theory of relativity hold relative to K. Let us suppose the same domain referred to a second body of reference K', which is rotating uniformly with respect to K. In order to fix our ideas, we shall imagine K' to be in the form of a plane circular disc, which rotates uniformly in its own plane about its centre. An observer who is sitting eccentrically on the disc K' is sensible of a force which acts outwards in a radial direction, and which would be interpreted as an effect of inertia (centrifugal force) by an observer who was at rest with respect to the original reference-body K. But the observer on the disc may regard his disc as a reference-body which is "at rest." The force acting on himself, and in fact on all other bodies which are at rest relative to the disc, he regards as the effect of a gravitational field.

The observer performs experiments on his circular disc with clocks and measuring rods. In doing so, it is his intention to arrive at exact definitions for the signification of time- and space-data with reference to the circular disc K', these definitions being based on his observations.

To start with, he places one of two identically constructed clocks at the centre of the circular disc, and the other on the edge of the disc, so that they are at rest relative to it. As judged from this body, the clock at the centre of the disc has no velocity, whereas the clock at the edge of the disc is in motion relative to K in consequence of the rotation. According to a result obtained in Chapter XII, it follows that the latter clock goes at a rate permanently slower than that of the clock at the centre of the circular disc, *i.e.* as observed from K. Thus on our circular disc, or, to make the case more general, in every gravitational field, a clock will go more quickly or less quickly, according to the position in which the

clock is situated (at rest). For this reason it is not possible to obtain a reasonable definition of time with the aid of clocks which are arranged at rest with respect to the body of reference.

If the observer applies his standard measuring rod (a rod which is short as compared with the radius of the disc) tangentially to the edge of the disc, then, as judged from the Galileian system, the length of this rod will be less than 1, since, according to Chapter XII, moving bodies suffer a shortening in the direction of the motion. On the other hand, the measuring rod will not experience a shortening in length, as judged from K, if it is applied to the disc in the direction of the radius. If, then, the observer first measures the circumference of the disc with his measuring rod and then the diameter of the disc, on dividing the one by the other, he will not obtain as quotient the familiar number $\pi = 3.14 \ldots$, but a larger number, whereas of course, for a disc which is at rest with respect to K, this operation would yield π exactly. This proves that the propositions of Euclidean geometry cannot hold exactly on the rotating disc, nor in general in a gravitational field, at least if we attribute the length 1 to the rod in all positions and in every orientation. Hence the idea of a straight line also loses its meaning. We are therefore not in a position to define exactly the co-ordinates x, y, z relative to the disc by means of the method used in discussing the special theory, and as long as the co-ordinates and times of events have not been defined we cannot assign an exact meaning to the natural laws in which these occur.

XXIV. EUCLIDEAN AND NON-EUCLIDEAN CONTINUUM

THE SURFACE of a marble table is spread out in front of me. I can get from any one point on this table to any other point by passing continuously from one point to a "neighbouring" one, and repeating this process a (large) number of times, or, in other words, by going from point to point without executing "jumps." We express this property of the surface by describing the latter as a continuum.

Let us now imagine that a large number of little rods of equal length have been made, their lengths being small compared with the dimensions of the marble slab. We next lay four of these little rods on the marble slab so that they constitute a quadrilateral figure (a square), the diagonals of which are equally long. To this square we add similar ones, each of which has one rod in common with the first. We proceed in like manner with each of these squares until finally the whole marble slab is laid out with squares.

If everything has really gone smoothly, then I say that the points of the marble slab constitute a Euclidean continuum with respect to the little rod, which has been used as a "distance" (line-interval). By choosing one corner of a square as "origin," I can characterise every other corner of a square with reference to this origin by means of two numbers. I only need state how many rods I must pass over when, starting from the

origin, I proceed towards the "right" and then "upwards," in order to arrive at the corner of the square under consideration. These two numbers are then the "Cartesian co-ordinates" of this corner with reference to the "Cartesian co-ordinate system" which is determined by the arrangement of little rods.

We recognise that there must also be cases in which the experiment would be unsuccessful. We shall suppose that the rods "expand" by an amount proportional to the increase of temperature. We heat the central part of the marble slab, but not the periphery, in which case two of our little rods can still be brought into coincidence at every position on the table. But our construction of squares must necessarily come into disorder during the heating, because the little rods on the central region of the table expand, whereas those on the outer part do not.

With reference to our little rods—defined as unit lengths—the marble slab is no longer a Euclidean continuum, and we are also no longer in the position of defining Cartesian co-ordinates directly with their aid, since the above construction can no longer be carried out.

If rods of every kind (*i.e.* of every material) were to behave *in the same way* as regards the influence of temperature when they are on the variably heated marble slab, and if we had no other means of detecting the effect of temperature than the geometrical behaviour of our rods in experiments analogous to the one described here, then our best plan would be to assign the distance *one* to two points on the slab, provided that the ends of one of our rods could be made to coincide with these two points.

The method of Cartesian co-ordinates must then be discarded, and replaced by another which does not assume the validity of Euclidean geometry for rigid bodies. The reader will notice that the situation depicted here corresponds to the one brought about by the general postulate of relativity.

XXV. GAUSSIAN CO-ORDINATES

ACCORDING TO Gauss, this combined analytical and geometrical mode of handling the problem can be arrived at in the following way. We imagine a system of arbitrary curves (see Fig. 4) drawn on the surface of the table. These we designate as u-curves, and we indicate each of them by means of a number. The curves $u = 1$, $u = 2$, and $u = 3$ are drawn in the diagram. Between the curves $u = 1$ and $u = 2$ we must imagine an infinitely large number to be drawn, all of which correspond to real numbers lying between 1 and 2. We have then a system of u-curves, and this "infinitely dense" system covers the whole surface of the table. These u-curves must not intersect each other, and through each point of the surface one and only one curve must pass. Thus a perfectly definite value of u belongs to every point on the surface of the marble slab. In like manner we imagine a system of v-curves drawn on the surface. These satisfy

the same conditions as the u-curves, they are provided with numbers in a corresponding manner, and they may likewise be of arbitrary shape. It follows that a value of u and a value of v belong to every point on the surface of the table. For example, the point P in the diagram has the

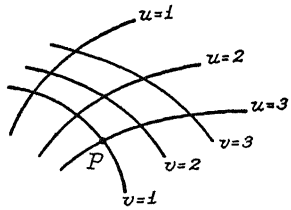

FIG. 4.

Gaussian co-ordinates $u = 3$, $v = 1$. Two neighbouring points P and P' on the surface then correspond to the co-ordinates

$$\begin{aligned} P&: \quad u,\ v \\ P'&: \quad u + du,\ v + dv, \end{aligned}$$

where du and dv signify very small numbers. In a similar manner we may indicate the distance (line-interval) between P and P', as measured with a little rod, by means of the very small number ds. Then according to Gauss we have

$$ds^2 = g_{11}\ du^2 + 2g_{12}\ du\ dv + g_{22}\ dv^2,$$

where g_{11}, g_{12}, g_{22} are magnitudes which depend in a perfectly definite way on u and v. The magnitudes g_{11}, g_{12}, and g_{22} determine the behaviour of the rods relative to the u-curves and v-curves, and thus also relative to the surface of the table.

For the case in which the points of the surface considered form a Euclidean continuum with reference to the measuring rods, but only in this case, it is possible to draw the u-curves and v-curves and to attach numbers to them, in such a manner, that we simply have:

$$ds^2 = du^2 + dv^2.$$

Under these conditions, the u-curves and v-curves are straight lines in the sense of Euclidean geometry, and they are perpendicular to each other. Here the Gaussian co-ordinates are simply Cartesian ones. It is clear that Gauss co-ordinates are nothing more than an association of two sets of numbers with the points of the surface considered, of such a nature that numerical values differing very slightly from each other are associated with neighbouring points "in space."

So far, these considerations hold for a continuum of two dimensions. But the Gaussian method can be applied also to a continuum of three, four, or more dimensions. If, for instance, a continuum of four dimensions be supposed available, we may represent it in the following way. With every point of the continuum we associate arbitrarily four numbers,

x_1, x_2, x_3, x_4, which are known as "co-ordinates." Adjacent points correspond to adjacent values of the co-ordinates. If a distance ds is associated with the adjacent points P and P', this distance being measurable and well-defined from a physical point of view, then the following formula holds:

$$ds^2 = g_{11}\,dx_1^2 + 2g_{12}\,dx_1\,dx_2 \cdot \cdot \cdot \cdot + g_{44}\,dx_4^2,$$

where the magnitudes g_{11}, etc., have values which vary with the position in the continuum.

We can sum this up as follows: Gauss invented a method for the mathematical treatment of continua in general, in which "size-relations" ("distances" between neighbouring points) are defined. To every point of a continuum are assigned as many numbers (Gaussian co-ordinates) as the continuum has dimensions. This is done in such a way that only one meaning can be attached to the assignment and that numbers (Gaussian co-ordinates) which differ by an indefinitely small amount are assigned to adjacent points. The Gaussian co-ordinate system is a logical generalisation of the Cartesian co-ordinate system. It is also applicable to non-Euclidean continua, but only when, with respect to the defined "size" or "distance," small parts of the continuum under consideration behave more nearly like a Euclidean system, the smaller the part of the continuum under our notice.

XXVI. THE SPACE-TIME CONTINUUM OF THE SPECIAL THEORY OF RELATIVITY CONSIDERED AS A EUCLIDEAN CONTINUUM

FOR THE TRANSITION from one Galileian system to another, which is moving uniformly with reference to the first, the equations of the Lorentz transformation are valid. These last form the basis for the derivation of deductions from the special theory of relativity, and in themselves they are nothing more than the expression of the universal validity of the law of transmission of light for all Galileian systems of reference.

Minkowski found that the Lorentz transformations satisfy the following simple conditions. Let us consider two neighbouring events, the relative position of which in the four-dimensional continuum is given with respect to a Galileian reference-body K by the space co-ordinate differences dx, dy, dz and the time-difference dt. With reference to a second Galileian system we shall suppose that the corresponding differences for these two events are dx', dy', dz', dt'. The magnitude

$$ds^2 = dx^2 + dy^2 + dz^2 - c^2\,dt^2,$$

which belongs to two adjacent points of the four-dimensional space-time continuum, has the same value for all selected (Galileian) reference-bodies. If we replace x, y, z, $\sqrt{-1}\,ct$, by x_1, x_2, x_3, x_4, we also obtain the result that

$$ds^2 = dx_1^2 + dx_2^2 + dx_3^2 + dx_4^2$$

is independent of the choice of the body of reference. We call the magnitude ds the "distance" apart of the two events or four-dimensional points.

Thus, if we choose as time-variable the imaginary variable $\sqrt{-1}\,ct$ instead of the real quantity t, we can regard the space-time continuum—in accordance with the special theory of relativity—as a "Euclidean" four-dimensional continuum.

XXVII. THE SPACE-TIME CONTINUUM OF THE GENERAL THEORY OF RELATIVITY IS NOT A EUCLIDEAN CONTINUUM

IN THE FIRST PART of this book we were able to make use of space-time co-ordinates which allowed of a simple and direct physical interpretation, and which, according to Chapter XXVI, can be regarded as four-dimensional Cartesian co-ordinates. This was possible on the basis of the law of the constancy of the velocity of light. But according to Chapter XXI, the general theory of relativity cannot retain this law. On the contrary, we arrived at the result that according to this latter theory the velocity of light must always depend on the co-ordinates when a gravitational field is present. In connection with a specific illustration in Chapter XXIII, we found that the presence of a gravitational field invalidates the definition of the co-ordinates and the time, which led us to our objective in the special theory of relativity.

We are led to the conviction that, according to the general principle of relativity, the space-time continuum cannot be regarded as a Euclidean one, but that here we have the general case, corresponding to the marble slab with local variations of temperature. Just as it was there impossible to construct a Cartesian co-ordinate system from equal rods, so here it is impossible to build up a system (reference-body) from rigid bodies and clocks, which shall be of such a nature that measuring rods and clocks, arranged rigidly with respect to one another, shall indicate position and time directly.

But the considerations of Chapter XXV and XXVI show us the way to surmount this difficulty. We refer the four-dimensional space-time continuum in an arbitrary manner to Gauss co-ordinates. We assign to every point of the continuum (event) four numbers, x_1, x_2, x_3, x_4 (co-ordinates), which have not the least direct physical significance, but only serve the purpose of numbering the points of the continuum in a definite but arbitrary manner. This arrangement does not even need to be of such a kind that we must regard x_1, x_2, x_3 as "space" co-ordinates and x_4 as a "time" co-ordinate.

The only statements having regard to these points which can claim a physical existence are in reality the statements about their encounters. In our mathematical treatment, such an encounter is expressed in the fact that the two lines which represent the motions of the points in question have a particular system of co-ordinate values, x_1, x_2, x_3, x_4, in common.

After mature consideration the reader will doubtless admit that in reality such encounters constitute the only actual evidence of a time-space nature with which we meet in physical statements.

The following statements hold generally: Every physical description resolves itself into a number of statements, each of which refers to the space-time coincidence of two events A and B. In terms of Gaussian co-ordinates, every such statement is expressed by the agreement of their four co-ordinates x_1, x_2, x_3, x_4. Thus, in reality, the description of the time-space continuum by means of Gauss co-ordinates completely replaces the description with the aid of a body of reference, without suffering from the defects of the latter mode of description; it is not tied down to the Euclidean character of the continuum which has to be represented.

XXVIII. EXACT FORMULATION OF THE GENERAL PRINCIPLE OF RELATIVITY

THE FOLLOWING STATEMENT corresponds to the fundamental idea of the general principle of relativity: *"All Gaussian co-ordinate systems are essentially equivalent for the formulation of the general laws of nature."*

If we desire to adhere to our "old-time" three-dimensional view of things, then we can characterise the development which is being undergone by the fundamental idea of the general theory of relativity as follows: The special theory of relativity has reference to Galileian domains, *i.e.* to those in which no gravitational field exists. In this connection a Galileian reference-body serves as body of reference, *i.e.* a rigid body the state of motion of which is so chosen that the Galileian law of the uniform rectilinear motion of "isolated" material points holds relatively to it.

In gravitational fields there are no such things as rigid bodies with Euclidean properties; thus the fictitious rigid body of reference is of no avail in the general theory of relativity. The motion of clocks is also influenced by gravitational fields, and in such a way that a physical definition of time which is made directly with the aid of clocks has by no means the same degree of plausibility as in the special theory of relativity.

For this reason non-rigid reference-bodies are used which are as a whole not only moving in any way whatsoever, but which also suffer alterations in form *ad lib.* during their motion. Clocks, for which the law of motion is of any kind, however irregular, serve for the definition of time. We have to imagine each of these clocks fixed at a point on the non-rigid reference-body. These clocks satisfy only the one condition, that the "readings" which are observed simultaneously on adjacent clocks (in space) differ from each other by an indefinitely small amount. This non-rigid reference-body, which might appropriately be termed a "reference-mollusk," is in the main equivalent to a Gaussian four-dimensional co-ordinate system chosen arbitrarily. Every point on the mollusk is treated as a space-point, and every material point which is at rest relatively to it as at rest, so long as the mollusk is considered as reference-body. The

general principle of relativity requires that all these mollusks can be used as reference-bodies with equal right and equal success in the formulation of the general laws of nature; the laws themselves must be quite independent of the choice of mollusk.

The great power possessed by the general principle of relativity lies in the comprehensive limitation which is imposed on the laws of nature in consequence of what we have seen above.

XXIX. THE SOLUTION OF THE PROBLEM OF GRAVITATION ON THE BASIS OF THE GENERAL PRINCIPLE OF RELATIVITY

FINALLY, the general principle of relativity permits us to determine the influence of the gravitational field on the course of all those processes which take place according to known laws when a gravitational field is absent, *i.e.* which have already been fitted into the frame of the special theory of relativity; it has also already explained a result of observation in astronomy, against which classical mechanics is powerless. According to Newton's theory, a planet moves round the sun in an ellipse, which would permanently maintain its position with respect to the fixed stars, if we could disregard the motion of the fixed stars themselves and the action of the other planets under consideration. Thus, if we correct the observed motion of the planets for these two influences, and if Newton's theory be strictly correct, we ought to obtain for the orbit of the planet an ellipse, which is fixed with reference to the fixed stars. This deduction, which can be tested with great accuracy, has been confirmed for all the planets save one. The sole exception is Mercury, the planet which lies nearest the sun. Since the time of Leverrier, it has been known that the ellipse corresponding to the orbit of Mercury, after it has been corrected for the influences mentioned above, is not stationary with respect to the fixed stars, but that it rotates exceedingly slowly in the plane of the orbit and in the sense of the orbital motion. The value obtained for this rotary movement of the orbital ellipse was 43 seconds of arc per century, an amount ensured to be correct to within a few seconds of arc. This effect can be explained by means of classical mechanics only on the assumption of hypotheses which have little probability and which were devised solely for this purpose.

On the basis of the general theory of relativity, it is found that the ellipse of every planet round the sun must necessarily rotate in the manner indicated above; that for all the planets, with the exception of Mercury, this rotation is too small to be detected with the delicacy of observation possible at the present time; but that in the case of Mercury it must amount to 43 seconds of arc per century, a result which is strictly in agreement with observation.

Apart from this one, it has hitherto been possible to make only two deductions from the theory which admit of being tested by observation,

to wit, the curvature of light rays by the gravitational field of the sun, and a displacement of the spectral lines of light reaching us from large stars, as compared with the corresponding lines for light produced in an analogous manner terrestrially (*i.e.* by the same kind of molecule).

PART THREE: CONSIDERATIONS ON THE UNIVERSE AS A WHOLE

XXX. COSMOLOGICAL DIFFICULTIES OF NEWTON'S THEORY

IF WE PONDER over the question as to how the universe, considered as a whole, is to be regarded, the first answer that suggests itself to us is surely this: As regards space (and time) the universe is infinite. There are stars everywhere, so that the density of matter, although very variable in detail, is nevertheless on the average everywhere the same.

This view is not in harmony with the theory of Newton. The latter theory rather requires that the universe should have a kind of centre in which the density of the stars is a maximum, and that as we proceed outwards from this centre the group-density of the stars should diminish, until finally, at great distances, it is succeeded by an infinite region of emptiness. The stellar universe ought to be a finite island in the infinite ocean of space.

This conception is in itself not very satisfactory. It is still less satisfactory because it leads to the result that the light emitted by the stars and also individual stars of the stellar system are perpetually passing out into infinite space, never to return, and without ever again coming into interaction with other objects of nature. Such a finite material universe would be destined to become gradually but systematically impoverished.

In order to escape this dilemma, Seeliger suggested a modification of Newton's law, in which he assumes that for great distances the force of attraction between two masses diminishes more rapidly than would result from the inverse-square law. In this way it is possible for the mean density of matter to be constant everywhere, even to infinity, without infinitely large gravitational fields being produced.

XXXI. THE POSSIBILITY OF A "FINITE" AND YET "UNBOUNDED" UNIVERSE

BUT SPECULATIONS on the structure of the universe also move in quite another direction. The development of non-Euclidean geometry led to the recognition of the fact that we can cast doubt on the *infiniteness* of our space without coming into conflict with the laws of thought or with experience (Riemann, Helmholtz).

In the first place, we imagine an existence in two-dimensional space.

Flat beings with flat implements, and in particular flat rigid measuring rods, are free to move in a *plane*. For them nothing exists outside of this plane: that which they observe to happen to themselves and to their flat "things" is the all-inclusive reality of their plane. In particular, the constructions of plane Euclidean geometry can be carried out by means of the rods, *e.g.* the lattice construction, considered in Chapter XXIV. In contrast to ours, the universe of these beings is two-dimensional; but, like ours, it extends to infinity. In their universe there is room for an infinite number of identical squares made up of rods, *i.e.* its volume (surface) is infinite. If these beings say their universe is "plane," there is sense in the statement, because they mean that they can perform the constructions of plane Euclidean geometry with their rods. In this connection the individual rods always represent the same distance, independently of their position.

Let us consider now a second two-dimensional existence, but this time on a spherical surface instead of on a plane. The flat beings with their measuring rods and other objects fit exactly on this surface and they are unable to leave it. Their whole universe of observation extends exclusively over the surface of the sphere. Are these beings able to regard the geometry of their universe as being plane geometry and their rods withal as the realisation of "distance"? They cannot do this. For if they attempt to realise a straight line, they will obtain a curve, which we "three-dimensional beings" designate as a great circle, *i.e.* a self-contained line of definite finite length, which can be measured up by means of a measuring rod. Similarly, this universe has a finite area that can be compared with the area of a square constructed with rods. The great charm resulting from this consideration lies in the recognition of the fact that *the universe of these beings is finite and yet has no limits.*

But the spherical-surface beings do not need to go on a world tour in order to perceive that they are not living in a Euclidean universe. They can convince themselves of this on every part of their "world," provided they do not use too small a piece of it. Starting from a point, they draw "straight lines" (arcs of circles as judged in three-dimensional space) of equal length in all directions. They will call the line joining the free ends of these lines a "circle." For a plane surface, the ratio of the circumference of a circle to its diameter, both lengths being measured with the same rod, is, according to Euclidean geometry of the plane, equal to a constant value π, which is independent of the diameter of the circle. On their spherical surface our flat beings would find for this ratio the value

$$\pi \frac{\sin\left(\frac{r}{R}\right)}{\left(\frac{r}{R}\right)},$$

i.e. a smaller value than π, the difference being the more considerable, the greater is the radius of the circle in comparison with the radius R of

the "world-sphere." By means of this relation the spherical beings can determine the radius of their universe ("world"), even when only a relatively small part of their world-sphere is available for their measurements.

Thus if the spherical-surface beings are living on a planet of which the solar system occupies only a negligibly small part of the spherical universe, they have no means of determining whether they are living in a finite or in an infinite universe, because the "piece of universe" to which they have access is in both cases practically plane, or Euclidean. It follows directly from this discussion that for our sphere-beings the circumference of a circle first increases with the radius until the "circumference of the universe" is reached, and that it thenceforward gradually decreases to zero for still further increasing values of the radius. During this process the area of the circle continues to increase more and more, until finally it becomes equal to the total area of the whole "world-sphere."

Perhaps the reader will wonder why we have placed our "beings" on a sphere rather than on another closed surface. But this choice has its justification in the fact that, of all closed surfaces, the sphere is unique in possessing the property that all points on it are equivalent. I admit that the ratio of the circumference c of a circle to its radius r depends on r, but for a given value of r it is the same for all points of the "world-sphere"; in other words, the "world-sphere" is a "surface of constant curvature."

To this two-dimensional sphere-universe there is a three-dimensional analogy, namely, the three-dimensional spherical space which was discovered by Riemann. Its points are likewise all equivalent. It possesses a finite volume, which is determined by its "radius" ($2\pi^2R^3$).

Suppose we draw lines or stretch strings in all directions from a point, and mark off from each of these the distance r with a measuring rod. All the free end-points of these lengths lie on a spherical surface. We can specially measure up the area (F) of this surface by means of a square made up of measuring rods. If the universe is Euclidean, then $F = 4\pi r^2$; if it is spherical, then F is always less than $4\pi r^2$. With increasing values of r, F increases from zero up to a maximum value which is determined by the "world-radius," but for still further increasing values of r, the area gradually diminishes to zero. At first the straight lines which radiate from the starting point diverge farther and farther from one another, but later they approach each other, and finally they run together again at a "counter-point" to the starting point. Under such conditions they have traversed the whole spherical space. It is easily seen that the three-dimensional spherical space is quite analogous to the two-dimensional spherical surface. It is finite (*i.e.* of finite volume), and has no bounds.

It follows, from what has been said, that closed spaces without limits are conceivable. From amongst these, the spherical space (and the elliptical) excels in its simplicity, since all points on it are equivalent. As a result of this discussion, a most interesting question arises for astrono-

mers and physicists, and that is whether the universe in which we live is infinite or whether it is finite in the manner of the spherical universe. Our experience is far from being sufficient to enable us to answer this question. But the general theory of relativity permits of our answering it with a moderate degree of certainty.

XXXII. THE STRUCTURE OF SPACE ACCORDING TO THE GENERAL THEORY OF RELATIVITY

According to the general theory of relativity, the geometrical properties of space are not independent, but they are determined by matter. Thus we can draw conclusions about the geometrical structure of the universe only if we base our considerations on the state of the matter as being something that is known. We know from experience that, for a suitably chosen co-ordinate system, the velocities of the stars are small as compared with the velocity of transmission of light. We can thus as a rough approximation arrive at a conclusion as to the nature of the universe as a whole, if we treat the matter as being at rest.

We already know from our previous discussion that the behaviour of measuring rods and clocks is influenced by gravitational fields, *i.e.* by the distribution of matter. This in itself is sufficient to exclude the possibility of the exact validity of Euclidean geometry in our universe. But it is conceivable that our universe differs only slightly from a Euclidean one, and this notion seems all the more probable, since calculations show that the metrics of surrounding space is influenced only to an exceedingly small extent by masses even of the magnitude of our sun. We might imagine that, as regards geometry, our universe behaves analogously to a surface which is irregularly curved in its individual parts, but which nowhere departs appreciably from a plane: something like the rippled surface of a lake. Such a universe might fittingly be called a quasi-Euclidean universe. As regards its space it would be infinite. But calculation shows that in a quasi-Euclidean universe the average density of matter would necessarily be *nil*. Thus such a universe could not be inhabited by matter everywhere; it would present to us that unsatisfactory picture which we portrayed in Chapter XXX.

If we are to have in the universe an average density of matter which differs from zero, however small may be that difference, then the universe cannot be quasi-Euclidean. On the contrary, the results of calculation indicate that if matter be distributed uniformly, the universe would necessarily be spherical (or elliptical). Since in reality the detailed distribution of matter is not uniform, the real universe will deviate in individual parts from the spherical, *i.e.* the universe will be quasi-spherical. But it will be necessarily finite. In fact, the theory supplies us with a simple connection between the space-expanse of the universe and the average density of matter in it.

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